A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. XII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR, FOR

THE AUTHOR,

AND SOLD BY HIM AT NO. 10, SOHO-SQUARE;

AND BY

H. D. SYMONDS, PATERNOSTER-ROW.

1805

PREFACE.

THE Authors of Original Papers in the present Volume, are, Davies Giddy, Esq. M. P.; Mr. O. Gregory; Mr. William Close; Mr. John Dalton; W. N.; Mr. Charles Young; Mr. J. Stodart; Count Rumford, V. P R. S.; Mr. G. I. Singer; J. P.; T. S. Traill, M. D.; G. Cumberland, Esq.; Mr. S. Clegg; Mr. Dalton; T. I. B.; T. Plowman, Esq.; F. A.; N. D. Starck, R. N.; John Gough, Esq.; A Correspondent; An Enquirer; Mr. Arthur Woolf; Mr. Elizur Wright; Thomas Northmore, Esq.; Dr. Beddoes; Mr. Bancks.

Of Foreign Works, Professor Proust; J. H. Hassenfratz; G. A. Lampadius; Mr. Prony; Beaunier; Gallois; J. B. Richter; Mr. Ritter; Citizen Bernoulli; Vauquelin; C. A. Prieur; Guyton; Morveau; Chaptal; Dr. Valli; M. Vaucher; M. Hauffman; Biot; Thenard; Descotils; C. L. Cadet; Professor Pfaff; Tromsdorf; M. Achard; M. Steinacher; M. Giobert; Humboldt; M. Lagoux de Flaix; M. Dodun.

And of English Memoirs abridged or extracted, Humphrey Davy, Esq. F. R. S.; Mr. Rose; Benj. Smith Barton, M. D.; Mr. John Heckewelder; J. C. Curwen, Esq. M. P.; Mr. William Bartram; Thomas Andrew Knight, Esq.; Rev. D. Pape; William Herschel, L. L. D. F. R. S.; Charles Hatchett, Esq. F. R. S.; T. C. Hope, M. D. F. R. S. Ed.

Of

PREFACE.

Of the Engravings, the Subjects are, 1. Apparatus for raising Water, by Mr. Close. 2. Furnace for bending Wood. 3, 4. Blowing Engine by the Fall of Water at Poullaouen. 5, 6. Apparatus for analysing Vegetable Soils, by H. Davy, Esq. 7. Diagrams to explain the Doctrine of Heat, by Count Rumford, V. P. R. S. 8. Magnified Representation of Insects found on Corn, by G. Cumberland, Esq. 9. An improved Steam Engine, by Mr. Clegg. 10. Simple Register Thermometers. 11. The Framing employed to raise the Roof of Clapham Church. 12. An improved Sheep Fold, by T. Plowman, Esq. 13. Applicative Compass, by Capt. Starck, R. N. 14. Section of a Drain, by J. C. Curwen, Esq. 15. Diagrams to illustrate a Series of Propositions respecting a Division of the Circle, by Mr. John Gough 16. Schemes exhibiting the Positions of the new Planet Juno, by Dr. Herschel. 17. Mr. Pape's Improvement of Rye Harbour. 18. A new Air Pump, by Mr. Wright of America. 19. New Valve and Parts of a condensing Syringe for the Gases, by Mr. Cuthbertson, with which Mr. Northmore's Experiments were made. 20. A very simple and improved Graphometer, by Mr. Bancks.

Soho Square, London, January 1, 1806.

ADVERTISEMENT.

THE Patrons and Friends of the Philosophical Journal are respectfully informed, that this work will in future consist of six sheets of matter instead of five in each number, together with a supplementary number to each volume; and that the plates will be so managed as to contain a larger number of subjects executed in the best stile. These improvements, though attended with additional expence, are such as the Editor has thought it his duty to adopt; in order that the very extensive communications with which the Journal has been honored, might not prevent him from giving all the foreign discoveries and such other general intelligence as the nature of the plan demands. It will easily be seen that the additional copy will by this means amount to one half more than the former quantity; and it is unnecessary to point out the great advantages which must result from such an addition. The Editor takes this occasion to repeat his acknowledgments for the encouragement which has been given to his exertions, particularly within the last twelve months, in which the sale has nearly doubled. The quantity of original matter continues to increase, and amounts to more than half of the whole work. Great part of the remainder consists in foreign articles never before published in this country, together with some extracts and abridgments from our best academical transactions. The whole publication may therefore be considered as original, since it is never made up by extracts from the periodical works of this country, which, on the contrary, very frequently copy from its contents.*

* It has been necessary to reprint a considerable part of the former volumes, in order to make complete sets, which may now be had, or any single numbers or volumes, from the commencement.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SEPTEMBER, 1805.

ARTICLE I.

Letter from DAVIES GIDDY, *Esq. M. P. describing a singular Fact of the invisible Emission of Steam and Smoke together from the Chimney of a Furnace; though either of them, if separately emitted, is visible as usual.*

To Mr. NICHOLSON.

SIR, *Clifton, August* 6, 1805.

Peculiar facts observed in working one of Trevithick's steam engines.

TRAVELLING, and a variety of occupations, have hitherto prevented me from sending you an account of the circumstances observed by myself and others, during the working of an engine on Mr. Trevithick's construction, at Merthyn Tidwell in South Wales, and which I had the pleasure of relating to you, some time since, in Soho Square. I now transmit a statement of the facts, avoiding all comments or attempts at explanation.

The steam engine was adapted to move a carriage, and every part made light and simple.

Mr. Trevithick having adapted his steam engine to the purpose of moving waggons, contrived every necessary part as light as he possibly could, and as little inconvenient to persons who might assist, or witness an experiment. The flue for conveying off the smoke, and affording a draft, was made of

of rolled iron; and the steam, which wholly escapes from these machines uncondensed, was conducted into the same tube, about a foot above its insertion into the boiler; therefore many feet from the fire, and beyond the register. When the engine began to move, it was soon remarked that neither steam nor smoke were seen to issue from the flue: and when fresh coal was added, nothing more than a faint white cloud became apparent, and that only for a short time; nor were drops or mist visible any where. It was proposed, that the register should be slowly closed; and as this was done, a condensation of steam manifested itself at a small distance from the chimney, and finally appeared in the same quantity, as if it had proceeded immediately from the boiler. The experiment was then reversed. The steam was gradually confined to the boiler; when smoke became more and more visible, till it equalled in quantity and appearance that commonly produced by a similar fire: and these trials were alternated a great number of times, with unvarying success. Lastly, it became a matter of speculation, whether or in what degree the draft was affected by the admission of steam into the flue. To ascertain this, every one present looked as attentively as possible into the fire-place; while the engine moved at the rate of a few strokes in a minute; and all agreed in declaring, that the fire brightened each time the steam obtained admission into the chimney, as the engine made its stroke.

The steam was (after work) thrown into the chimney many feet from the fire.

Neither smoke nor steam were visible.

When the smoke was shut off the steam was visible, and when the steam was shut off the smoke was visible.

The draft up the chimney was increased by the admission of the steam.

I am, Sir,

Your very faithful humble servant,

DAVIES GIDDY.

II.

On a Meteoric Stone that fell in the Neighbourhood of Sigena, in Arragon, in 1773, by Professor PROUST*

THE author begins his paper by some previous historical facts. No one now questions, that stones have fallen from the atmosphere in different parts of the world. The ancients

Stones have fallen from the atmosphere in various ages,

* Abridged from the Journal de Physique, for March 1805.

mention

mention it as having occurred at various times, and later ages have recorded the time and authenticated circumstances of several such incidents. In our own days, stones or mineral bodies, termed meteoric, have been collected in the East Indies, America, Scotland, England, France, Italy, Hungary, and lastly in Spain: and that nothing may be wanting in future to convince those who refuse their assent to the united testimony of all ages and all countries, nature appears to have purposely ordered a repetition of this surprising phenomenon: no longer ago than the 26th of April, 1803, a shower of these stones covered a space of ground two miles long and above a mile broad, near l'Aigle in Normandy. The French Institute immediately nominated a commissioner, to examine into the fact on the spot, to take the depositions of witnesses, compare them with the circumstances, and bring some of the stones to Paris.

and in all parts of the world.

Shower of stones in 1803.

As the first thing to be done with a new mineralogical substance, is, to analyse it, the President of the Royal Society of London, and several other gentlemen who had such stones in their collections, put them into the hands of Mr. Howard, a Member of the Society, that he might subject them to chemical examination. He found to his great surprise, that all these stones, from the remotest quarters of the globe, contained the same principles, differing only in proportion; and, what was still more striking, that they all contained iron combined with nickel, a compound to be met with among none of the minerals in any part of the globe with which we are acquainted. Vauquelin has since confirmed by repeated experiments, the accuracy of Mr. Howard's observations. All men of science have hence been led to conclude, that these stones must have a common origin; but whence they originate is the question. Do they belong to that earth on which they fall? are they formed in the atmosphere itself? or have they been projected from lunar volcanoes? On these points men's sentiments are divided; and the arguments have been collected by Dr. Izarn, in his *Lithologie Atmosphérique*.

Analysis of many of these bodies by Mr. Howard;

who found them all similar in their composition.

They have therefore a common origin,

but whence?

One of these stones has been in the royal collection at Madrid ever since 1773. This the minister has allowed Mr. P. to analyse, leaving the principal part of it still in the collection for the satisfaction of the curious. The following letter was sent with it to Don Manuel de Roda, Minister of State, by the captain-general of Saragossa.

One in the collection at Madrid.

 "In

Account of its fall.

" In November laſt an extraordinary occurrence, ſaid to have happened on the Seventeenth of that Month in a ploughed field at Sena, a village in the diſtrict of Sigena, was the ſubject of converſation in this city.

" The ſky being perfectly ſerene, three reports reſembling thoſe of cannon were heard, and followed by the fall of a ſtone weighing nine pounds and one ounce, at a little diſtance from two labouring men. One of them went up to it, but the ſtrong ſmell it emitted ſtopped him a moment.

" Recovering from his ſurpriſe he went nearer, raiſed it up with his ſpade, and waited till it was ſufficiently cold for him to carry it to the village, where he delivered it to the prieſt.

" From inquiries made immediately afterwards on the ſpot, and among the people in the neighbourhood, it appears, that the noiſe in the air and fall of the ſtone were not accompanied with any ſtorm, or with lightning."

Another meteoric ſhower of ſtones that fell at Roa in Spain, in 1438,

To bring into one view all that is yet known of ſtones falling in Spain, the author ſubjoins a letter of the Bachelor Cibdadreal, on thoſe that fell in the village of Roa, near Burgos, in 1438.

in the view of the King of Spain.

" While the King Don John and his court were hawking near the village of Roa, the ſun was concealed behind white clouds, and bodies reſembling gray and blackiſh ſtones were ſeen to fall from the air, of ſuch bulk as to occaſion the greateſt ſurpriſe.

The ground was covered with them.

" After this phenomenon had continued for an hour the ſun re-appeared, and the falconers immediately rode to the place, which was not above a mile diſtant. They brought back information to the king, that the ground was ſo completely covered with ſtones of all ſizes as not to be viſible.

" The king would have gone thither, but his courtiers prevented him, obſerving, that a place choſen by Heaven for the theatre of its operations might not be free from danger, and that he had better ſend ſome of his attendants. Gomez

Theſe ſtones were very large,

Bravo, the captain of his guards, undertook the office. He brought four of the ſtones to Roa, whither the king had already retired. They were of conſiderable ſize: ſome were round, and as large as a mortar, others like pillows and *half-fanegue*

extremely light,

meaſures (ſuch as contain about 45lbs weight of corn,) but what was moſt aſtoniſhing, was their exceſſive lightneſs, ſince

the largeſt did not weigh half a pound. They were of ſuch a tender texture, that they reſembled the foam of the ſea condenſed more than any thing. You might ſtrike on them with your hand without fear of bruiſe, or pain, or the ſlighteſt mark.—The king has ordered ſome to be ſent you, &c."

and of a tender texture.

It ſeems from this deſcription, that theſe ſtones muſt have been very different from thoſe of the preſent day.

The ſtone of Sigena deſcribed.

The ſtone of Sigena, when delivered to Mr. Prouſt, weighed ſix pounds ten ounces. With it was a piece of three or four ounces, the only one remaining of thoſe that had been broken from it by curious perſons. It was interſperſed with ſpots of ruſt, both externally and internally, owing probably to its having been immerſed in water to try the effect of that fluid on it. From theſe however, ſome inſtructive inferences may be drawn reſpecting the native place of theſe ſtones.

Interſperſed with ruſty ſpots.

Its figure.

Its ſhape is an irregular oval, ſeven or eight inches long, four or five broad, and four in its greateſt thickneſs. One ſide is flattiſh, a little depreſſed in the middle, and very round on the edges: the other is an obtuſe triedral pyramid with unequal ſides, greatly rounded at the ſummit and on the edges*. It appears to have had the black vitreous cruſt common to ſtones of this kind, though from its fragility the greater part has fallen off in paſſing through many hands and receiving occaſional blows, ſo that none remains except in the hollow of the baſe, and a little on the faces of the pyramid.

It had a vitreous cruſt.

On examining this cruſt it is eaſy to ſee, that it muſt have been the effect of heat ſubſequent to the formation of the ſtone, and unqueſtionably very powerful though momentary; ſince the metallic and ſulphureous particles immediately beneath the cruſt had not time to change colour, or even loſe their luſtre.

This muſt have been produced by violent momentary heat ſince its formation.

It has all the poroſity of an aggregated maſs of ſandy particles without any cement, ſo that the breath will eaſily paſs through a piece held between the teeth. It will not ſtrike fire with ſteel, and the ſame may be ſaid of the pyrites it contains.

It is porous, not very hard,

Its colour is a uniform bluiſh gray, like that of a black ſubſtance enlightened by a white: it is the hue of an earthy compound tinged by iron oxided at a *minimum*.

of a bluiſh gray.

* Does this deſcription agree with what is ſaid above, that ſeveral pieces have been broken from it? Apparently above a quarter of the ſtone, on comparing its original and preſent weight. J. C.

The

It is a sandy mass interspersed with metallic and sulphureous particles. The stone itself is a sandy mass, formed of rounded oval grains, the largest of which are scarcely bigger than hempseed, among which are interspersed metallic and sulphureous particles with all their primitive lustre, and particularly with that light tint of kupfernickel observed in the other stones.

Its granules are crystalline. On examining the earthy grains by the microscope, we perceive, that, far from having been fashioned by the movement of water, they are globules rough with crystalline or reflecting points, so that they can by no means be confounded with sand.

Heat deepened its colour, and oxided the metal. A piece of about two inches being exposed to a red heat in a crucible for half a quarter of an hour was much changed: the sandy globules became of a darker gray, and the metallic particles, divested of their lustre, were visibly oxided.

G[illegible]er heat fused it. About two ounces were heated for half an hour in a forge fire, which converted the stone into a semivitreous mass, blackish, and slightly porous. It did not appear to have effervesced much previous to fusion, and was interspersed with globules of iron, which had not time to descend, though upwards of a hundred grains of a regulus were collected at the bottom.

It contained much magnetic iron, The iron attractable by the magnet was not uniformly mixed in the stone, as from some parts 22 in the 100 were extracted, from others not more than 17.

combined with nickel. This iron was combined with nickel in the proportion of about 3 per cent. No nickel was discoverable in any other part of the stone.

After this alloy was separated by the magnet, the remainder of the stone was found by analysis to consist of,

Constituent parts of the remainder.

Iron sulphurated at a *minimum* -	12
Black oxide of iron - - -	5
Silex - - - - -	66
Magnesia - - - -	20
Lime and magnesia in quantities too small to be appreciated - -	
	103

New hypothesis. Their origin probably in the polar regions, On considering the rapid alteration of these stones by moisture, for a fragment kept twelve hours under water was taken out covered with spots of rust, which distinguished the grains of alloy from the sulphureous particles with which they were before confounded;—it is obvious, according to the author

author, that they cannot subsist in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a solid mass, and iron cannot rust, he thinks we may reasonably look to these regions as the native place of such bodies. In this he insists there is nothing impossible, or even improbable. And why should those meteors, he demands, of which we know neither the origin, the combustibles that afford them aliment, the impulse by which they are moved, nor the nature of the lines they describe in their course, be less capable of tearing them from some part of the globe, than of forming them, contrary to all physical probability, from elements which the atmosphere can neither create nor hold in solution? whence they are conveyed to other parts by meteors.

III.

Further Remarks on Mechanic Power, in Reply to Mr. J. C. Hornblower. In a Letter from Mr. O. Gregory, Royal Mil. Academy.

To Mr. NICHOLSON.

SIR,

I AM sorry to be under the necessity of troubling you with a few observations for insertion in your Journal, in consequence of being called upon by Mr. Hornblower, as though it were to defend some newfangled doctrine, when the positions in my former letter, which that gentleman thinks proper to censure, are in perfect conformity with the principles assumed or demonstrated by every correct writer on mechanical philosophy since it has been placed upon its proper basis in the *Principia* of Newton. The subject I am now invited to discuss, has so frequently been exhibited in the clearest light by various authors, both in England and on the Continent, that I should not think myself justified in occupying many of your pages by an elaborate dissertation; out of regard, however, to so respectable a correspondent as Mr. H. I cannot help entering a little into the discussion, though I am, I confess, quite unable to ascertain whether his last letter is meant to oppose my former remarks, and those of Professor Robison with serious arguments, or is merely intended as a *Jeu d'esprit*. Prefatory remarks.

It

It will not, I hope, be expected that I should point out in what instances Mr. H's remarks appear to me completely irrelative to the subject in hand; or those in which he seems to have misunderstood the arguments of the late leared Professor: such a procedure would only lead into farther discussion, while I feel solicitous to avoid it, from a consciousness that it would be very uninteresting to most of your readers. I shall strive to confine myself, therefore, to such of Mr. H's enquiries as bear upon the point in dispute, and for the sake of condensing my labour, shall begin with that in his postscript.

Are animal exertion and mechanic power identical?

First then, I will endeavour to "set Mr. H. right as to the identity of *animal exertion* and *mechanic power*:" and to this end it will be requisite to answer the question,—what *is* mechanic power? excluding, for the present, that acceptation of the term in which it is understood to denote one of the six simple machines. Now, it is pretty obvious, that the terms power, force, &c. when used in mechanical science are purely metaphorical; for, as Professor Dugald Steward remarks, (*Elements of the Philosophy of the Human Mind*, p. 202.) "All the languages which have hitherto existed in the world, have derived their origin from *popular use*; and their application to philosophical purposes was altogether out of the view of those men who first employed them." Language commenced amongst simple men, who had little, if any acquaintance with what is now called science: and in the gradual progress of most nations, from the savage to the shepherd state, thence to the agricultural, and farther to the commercial state, it would be very long before they would think of attaching any other meaning to the terms in the different languages, corresponding to power, force, action, resistance, repulsion, &c. than those which were manifestly referable to the different kinds of human, or of animal exertion; in subsequent times when scientific men began to classify, arrange, and systematize the phenomena which they observed in the congress, motion, and mutual operation of bodies, they found it much easier to denote the circumstances they would describe of treat of, by a figurative application of old terms, to which some analogous notions would necessarily be attached by every person, than to invent new ones, which would be attended by no ideas independant of an arbitrary definition. Nor, in this application, was there any danger of important

Rise and application of the terms, force, power, &c.

error

error, for the things to which the terms were appropriated would exhibit such specific differences as would almost entirely preclude the chance of confounding one with another; and leave no room to fear, that when the terms were applied to inanimate beings, they would be concluded to exert strength, or to possess power, as animals did; any more than we should now fear being misunderstood when we speak of the force of arguments, the attractions of benevolence, the fascinations of beauty, or the repulsive tendency of envy. Thus, from contemplating the process of this gradual refinement, (a refinement produced not by barren speculators, but by the necessary demands occasioned by the progress of civilization,) we see that the words power and force, primarily used to denote animal energy, are now, by a natural extension grounded upon an obvious analogy, employed to express *efficiency* in general. It will hence be easy to assign the proper philosophical acceptation of these terms, when used in the science of mechanics. *Force or power, in a mechanical sense, is that, whatever it be which causes a change in the state of a body, whether that state be rest or motion.* This definition does not require our entering into any metaphysical disquisitions relative to the nature of causes, or the connection of cause and effect: that every event is brought about by some cause, that is, by some agency, or something which precedes in the order of occurrence, is a truth which I think none will be disposed to deny; but what is the agency, or where it actually resides, we can seldom know, except perhaps in the case of our own voluntary actions. It is not then the business of the mechanist, strictly speaking, to enquire into the *modus operandi*; we learn from universal experience, that the muscular energy of animals, the operation of gravity, electricity, impact, pressure, &c. are sources of motion, or of modifications of motion; and hence, without pretending to know any thing of the essence of either of these, we do not hesitate to call them mechanical forces; because it is incontrovertible that bodies exposed to the free action of either, are put into motion, or have the state of their motion changed. Forces therefore, being only known to us by their effects, can only be measured by the effects they produce in like circumstances, whether those effects be creating, accelerating, retarding, deflecting, or preventing motions: and it is by comparing these effects, or

Definition of the terms force and power.

Forces only known to us by their effects, thence measureable.

or by referring them to some common measure of ready appreciation, that mechanics is made one of the mathematical sciences *.

Animal efforts are a species of mechanic power.

These observations will enable us to set Mr. H. "right as to the identity of *animal exertion* and *mechanic power*." Animal efforts are justly considered, both by the Mathematician and the practical Engineer, as constituting *one species* of mechanic power; when these efforts give to bodies equal momenta, or give to equal bodies equal velocities, it is truly said the animals exert equal forces; and we say that animal power is greater or less, as it is capable of imparting to bodies greater or less momenta, or as it is capable of stopping bodies moving with greater or less momenta: and the language of scientific men is analogous to this when they speak of any forces whatever.

Reply to Mr. H's remarks on a former statement.

It is now time to proceed to Mr. Hornblower's animadversions upon the instance I adduced (and many others might be adduced) to shew that Mr. Smeaton's measure of mechanical power and effect is not universally applicable. I asserted,

* That Mr. Hornblower may not rest upon the mere authority of any theoretic man, I beg to throw into this note an extract from the *Méchanique Philosophique* of M. Prony, an Engineer, who unites with a profound acquaintance with the theory, an extensive practice, and whose example in this respect I should sincerely rejoice to see more frequently imitated in this country.

"La nature de cette cause de mouvement, nommée *force* ou *puissance*; nous est tout-á-fait inconnue: l'homme appelle *force* la faculté organique qu'il a de se mouvoir, de s'arrêter, de produire ou de faire cesser le mouvement des corps qui l'environnent; et sans savoir en quoi consiste cette faculté, il a supposé qu'il existait quelque chose de semblable dans les agens physiques qui sont ou qu'il croit être, sur le globe terrestre et dans l'univers, les causes du mouvement de différens corps. Mais nous n'avons en mecanique, aucun besoin de connaître la nature de la *force* ou *puissance* qui est représentée, mesurée et introduite, dans le calcul, uniquement par les *effets* qu'elle produit. Ces effets se réduisent toujours à des vétesses que les puissances ou tendent à donner, ou ont effectivement données à de certaines masses." "Parmi les diverses puissances que la nature nous offre, il en est une très remarquable dont il convient de prendre les effets pour terme de comparaison de ceux des autres puissances; c'est la *pesanteur terrestre* a la surface de la terre," &c. p. 20.

that

that a horſe ſtanding ſtill and ſuſtaining a weight which hung by a cord over a fixed pulley, would, after a due interval of time, be completely fatigued, although neither the animal nor the weight moved, and that, of conſequence, there was a power expended, of which Mr. Smeaton's rule did not furniſh an adequate meaſure. Mr H. as though he underſtood me to affirm, that fatigue was the *only* indication of mechanical power expended, inſtead of limiting it to animal efforts as the connection evidently required, exclaims, "it is really difficult to be grave on this occaſion:" p. 268. and argues in a kind of exulting ſtrain which ſavours a little, I am afraid, of the fault alluded to in the French proverb *Chanter le triomphe avant la victoire!* Let us, ſays this gentleman, have a "*poſt* inſtead of the horſe, and ſurely that will not tire, and what will be the conſequence then? why then there will be no power expended, and no effect produced." Mr. H. then, it would ſeem, has forgotten that the poſt is retained in *its* ſituation by a force which in this caſe oppoſes that of gravity acting upon the ſuſpended weight. The cord running over the pulley and ſuſtaining the weight, being faſtened to the poſt would move it, were it not that the coheſive force of the earth in which the poſt is fixed, changes the ſtate into which the poſt would be brought by the action of gravity upon the weight, and is ſufficient to retain the whole at reſt. If the poſt were ſet in looſe ſand, or in a quagmire, the weight would draw it away, and then I ſuppoſe, even according to Mr. H's notion, there would be a power expended, and an effect produced. So likewiſe, in Mr. H's other example, of the hat hung upon the pin, the force of gravity is balanced by the coheſive force of the wood or other matter, into which the pin is fixed. But it would be egregious trifling to dwell much longer upon ſuch inſtances as theſe. Mr. H. conceives, if I have not completely miſunderſtood his meaning, that there is no "mechanical power" that is not "made up of a maſs of matter moving with a determinate velocity;" and as ſuch an opinion muſt either ariſe from neglecting to diſcriminate between cauſe and effect, or from a virtual denial of the whole doctrines of ſtatics, (in which powers are excited without any motion being produced,) I ſhall hope to be excuſed though I waſte no time on a refutation of any ſuch poſition.

Reply to Mr. H's objection to my former ſtatement.

Indeed,

Indeed, much of Mr. H's reasoning, not only with respect to the post and the hat-peg, but throughout his paper, seems to rest upon a tacit admission (not a direct avowal, it is true,) of the erroneous notion, that forces exerted by animated beings, and those operating through the intervention of inanimate things, are totally distinct, and cannot be substituted the one for the other, or have a fair comparison instituted between them. Whereas, on the contrary, not only the theory but the practice of mechanics, proceeds upon the principle, that those forces are equal in degree, however different in their origin, or various in their mode of operation, which produce equal effects. Thus, for a familiar example, in the boring of a piece of ordnance; the borer may either be brought up to its proper position in the gun by the action of a man on the handles of a wheel, connected with the borer by rack and pinion work, or by the action of a weight attached to the farther end of a lever proceeding from the axle of the same wheel: and Mr. H. might as well deny the possibility of the work performed being the same in both these cases, as deny that a weight is kept from falling by an equal force, when prevented either by an animal, or by a fixed inanimate object; or deny that there is an expenditure of mechanic power when a man counteracts the operation of gravity upon his arm, when extended horizontally. While speaking of the proposition which includes any such denial, we may safely apply to it Mr. H's own language;—"A more erroneous proposition was never introduced into the theory or practice of mechanics."

Familiar illustration.

Mr. Hornblower has taken the trouble of extracting several passages from the Article MACHINERY, *Sup. Ency. Britan.* and among them has taken that which exhibits Professor Robison's measure of the exertion of a man, who walks at the rate of 60 feet per minute, and raises a weight of 30 pounds. The measure 57600, which this gentleman thinks enormously too large, is, in fact, too small, in so far as it does not include that part of the exertion required by the man to move himself. It was this omission of the learned Professor that induced me to lay down the general statement at p. 152 of your 43d Number, though I thought it might be deemed invidious if I specified my motives in that place. But when Mr. H. had commenced the labour of extracting, it would

Mistake of Professor Robison.

surely

surely have been but candid to produce a passage which strikes with great force against the universality of Mr. Smeaton's measure, at the same time that it admits the utility of this measure to engineers in many cases. This passage is as follows:—"When a weight of five pounds is employed to drag up a weight of three pounds, by means of a thread over a pulley, it descends with a motion uniformly accelerated, four feet in the first second. Mr. Smeaton would call this an expenditure of a mechanical power 20. The weight three pounds is raised four feet. Mr. Smeaton would call this a mechanical effect 12. Therefore the effect produced is not adequate to the power expended. But the fact is, that the pressure, strain, or mechanical power, really exerted in this experiment, is neither five nor three pounds; the five pound weight would have fallen 16 feet, but it falls only four. A force has therefore acted on it sufficient to make it describe 12 feet in a second, with a uniformly accelerated motion, for it has counteracted so much of its weight. The thread was strained with a force equal to $3\frac{3}{4}$ pounds, or $\frac{3}{4}$ of 5 pounds. In like manner, the three pound weight would have fallen 16 feet; but it was raised four feet. Here was a change precisely equal to the other. A force of $3\frac{3}{4}$ pounds acting on a mass whose matter is only three, will in a second, cause it to describe 20 feet with a uniformly accelerated motion. Now 5×12 and 3×20, give the same product 60. And thus we see, that *the quantity of motion extinguished or produced, and not the product of the weight and height, is the true unequivocal measure of mechanical power really expended, or the mechanical effect really produced; and that these two are always equal and opposite.* At the same time, Mr. Smeaton's theorem merits the attention of engineers; because it generally measures the opportunities that we have for procuring the exertion of power. In *some* sense, Mr. Smeaton may say, that the quantity of water multiplied by the height from which it descends in working our machines, is the measure of the power expended; because we must raise this quantity to the dam again, in order to have the same use of it. It is expended, but not employed, for the water at leaving the wheel is still able to do something."

Robison on Smeaton's measure.

In opposition to all this, Mr. H. I suppose, would say that this is not a case in point, because, "if the weight descends quickly

Messrs. Hornblower and Smeaton both concede the point in debate?

quickly it is ſenſibly compounded with another law, viz. the acceleration of gravity." Or, adopting other language of Mr. Smeaton, he might reſtrict his meaſures to "the height through which a body *ſlowly* and *equally* deſcended, or to which it was raiſed." But if, inſtead of the body's aſcending or deſcending ſlowly and equally, it moved rapidly and irregularly; or, if the motion was reciprocating, the velocity increaſing from quieſcence to a certain magnitude, and diminiſhing to quieſcence again; or, if we refer to the retarded riſe and accelerated fall of heavy ſtampers; in ſuch caſes if Smeaton's meaſure be applicable, I wiſh to ſee its manner of application explained: and if it be not univerſally applicable, a point which is, in reality conceded both by Mr. H. and Mr. Smeaton, there is then as to this head no ground of difference between us, and Mr. H's laſt letter becomes in a great meaſure a ſuperfluous labour; for, admitting the want of univerſality in the rule, is admitting all that I affirmed. Had not the meaſure been often very injudiciouſly exhibited as *univerſal*, a thing which Mr. Smeaton himſelf certainly never intended, I ſhould not have at all referred to it in my former paper.

In correct language *weight* and *heavineſs* be diſtinguiſhed.

It may be deemed a ſlight deviation from the immediate object of this letter, but I truſt a juſtifiable one, if I briefly notice the ſurprize expreſſed by Mr. H. on account of Profeſſor Robiſon's diſtinguiſhing between weight and heavineſs. That the three terms *gravity*, *weight*, and *heavineſs*, admit of a palpable and obvious diſtinction, is, in my opinion, indubitable: and till this time, I imagined it was univerſally reckoned one great excellence of an accurate philoſophical diſquiſition, that it compriſed a careful diſcrimination of the various acceptations of theſe and other terms, which were commonly reputed ſynonimous. There may, undoubtedly, be occaſions in which a cautious ſelection from words of nearly ſimilar import may be diſpenſed with: but there are many more, particularly when handling philoſophical topics, where this careful choice cannot be ſafely neglected. And an attention to this point appears the more neceſſary, when it is recollected, that greater part of the controverſies which have been agitated by men of ſcience, have been rather verbal, than relative to things in themſelves. To contend for the uſe of many terms to expreſs one idea, inſtead of ſeeking

for

for adequate feparate expreffions to denote every idea the mind can form, is to facrifice precifion and accuracy at the fhrine of an ill-judged fuperfluity. The common refemblance between words efteemed fynonimous, does not comprehend the aggregate fignification, but fome ifulated particular attendant upon all, in fome fuch manner as may be traced in individuals of the fame fpecies: there is generally one, if not more qualities, on which a manifeft diftinction depends; and the determination of fuch qualities is highly deferving the notice, not only of the linguift, but of all who aim at philofophical precifion. I have not leifure to look attentively over twenty-five clofely printed quarto pages, in order to find how Profeffor Robifon diftinguifhes heavinefs from weight. But the labour is unneceffary; for the diftinction has often been made; and I will take the liberty of delineating it in the words of an author who is in no danger of having his fentiments warped to fquare with the tenets of any fpeculative mechanical fyftem: I now advert to Dr. Trufler, who in his work on fynonimous words fpeaks thus:—

Trufler's remarks on heavinefs and weight.

"*Heavinefs, weight.*—In the figurative fenfe the difference of thefe words is fo extremely great, as needs no pointing out; in the literal indeed, they are often confounded: confidered then in this laft fenfe, *heavinefs* is that quality in a body which we feel, and diftinguifh by itfelf: *weight* is the meafure and degree of that quality, which we cannot afcertain but by comparifon.—We fay abfolutely, and in an undetermined fenfe, that a thing is *heavy*: but relatively, and in a manner determined, that it is of fuch a *weight*, for example, of two, three, or four pounds.—A thoufand circumftances prove the *heavinefs* of the air; and the mercury determines its exact *weight*." Vol. I. p. 133.

My letter has attained a much greater magnitude than was at firft intended, and I will now conclude it. The remarks I have been tempted to offer, are founded upon the moft correct interpretation I could put upon Mr. Hornblower's language; and if I have any where mifunderftood his meaning, I fhall be pleafed to fee that mifunderftanding candidly removed. I entertain great refpect for that gentleman's talents as a practical engineer; though I cannot but think him completely wrong in moft of thofe remarks which have occafioned this communication. I have replied to fuch of his

ftrictures as bore any relation to myfelf, I believe, without acrimony: but I have a deeply rooted averfion to every thing that wears the garb of controverfy, and ardently hope the difcuffion on my part will be permitted to terminate here.

I am, Sir,

Your's, with much refpect,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich, Aug. 9, 1805.

IV.

Defcription and Effects of an Apparatus for raifing Water by Means of Air condenfed in its Defcent through an inverted Syphon. By Mr. WILLIAM CLOSE. *From the Inventor.*

To Mr. NICHOLSON.

SIR, *Dalton, July* 27, 1805.

Reference to the author's fyphon engine.

IN one of my letters, fome time ago, I briefly noticed an experiment I had made, to determine the practical value of the hydraulic machine, or inverted fyphon, reprefented and defcribed in the firft volume of the prefent feries of your Journal *, obferving, that, at fome future period, I might probably tranfmit to you a more particular account. Having fince repeated the experiment, I now fend you a letter upon the fubject, for I am of opinion that a machine operating upon the principle, when conftructed in the manner herein defcribed, will anfwer very well, in certain fituations, to raife water for domeftic purpofes; and although it may not be competent to perform half as much work as a bucket engine by a forcing pump, yet it may be kept continually employed, and be fubject to very little wear, as its operation will almoft be performed without friction.

Defcription of another apparatus;

The inverted fyphon when applied to raife water in the manner defcribed in this letter, has its higher orifice placed in a fituation to receive both air and water at the fame time. The air being conveyed by the velocity of the aqueous column

* See Philof. Journal, Vol. I. p. 30, Pl. IV.

to

to the lowest part of the syphon, and collected in a vessel, is employed as the medium for conveying pressure to raise water in another part of the apparatus.

In May 1803, I determined to find by experiment, under what degree of pressure it would be most advantageous to collect the condensed air, and likewise the proportion then existing between the two fluids moving in the syphon. The apparatus constructed for this purpose, is represented in *Plate I. Fig.* 1. It required only a small supply of water, but condensed the air sufficiently to be employed in the actual construction of a machine upon the principle.

being an inverted syphon, which carries down air and condenses it;

A round vertical pipe A B, half an inch in diameter, and 22 feet 5 inches in length, had its higher end placed in the cistern A, and its lower connected to a small oblong vessel C, which had an inverted glass bottle cemented upon a projecting cylinder on its upper side. From the other end of the vessel ascended another vertical pipe D E, half an inch in diameter, and 18 feet 3 inches in length, and terminated in a crook, 4 feet 2 inches below the highest part of the pipe A B.

The whole apparatus being filled with water, the cistern having a constant supply sufficient to keep the surface of the fluid just above the orifice of the pipe A B, when the orifice of emission at E was opened, the water flowing through A B, carried bubbles of air into the vessel C, which ascending, displaced the water in the bottle, and afterwards that contained in the vessel C, above the lower ends of the pipes A B and D E. At the first efflux, and after the descent of every material portion of air, the jet at E was projected several inches from the adjutage, but its curve decreased during the descent of more air; for the bubbles did not rise incessantly into the bottle, but after short intervals of rest, dislodging two or three ounces of water each time, with a guggling noise, which was very audible to the person regulating the supply of the cistern. After the water in the vessel C was depressed to a level with the ends of the pipes, the dense air carried down A B, ascended through D E, and caused frequent interruptions in the jet; for, expanding under a light pressure, it expelled the water in the highest part of the pipe with violence, and then the efflux ceased for some time after.

and this air is afterwards used for raising water.

 The

The condensed air, however, could any time be let out, by a small pipe which was placed within the bottle, and opened on the outside of the vessel C.

The pipe A B had a joint above the bottom of the cistern, to facilitate the trial of mouth-pieces of various forms, to find by which the apparatus would sip the most air: and it appeared that no form, or position, conduced more to this effect, than when the pipe was crooked at top to receive the water in a horizontal current, and the higher side of its orifice was not more than two lines below the surface of the water in the cistern. It also appeared, that no less quantity of air was collected, when the diameter of the orifice of emission was reduced to four lines, than when it was half an inch.

Experiments to shew how much air can be carried down with he water.

After several experiments to determine the quantity of water requisite to supply the expenditure from the cistern, and keep the surface of the fluid accurately at the height best adapted to the operation of the apparatus; several trials were made to ascertain the quantity of air a given quantity of water would convey into the bottle in a given time. The results of several trials on the 21st of May 1803, were as follow:

1. The fall being 50 inches, and the orifice of emission four lines in diameter, the inverted bottle above C, holding ten ounce measures of water, was filled with air, under the pressure of a column 18 feet high, by 14 pints of water flowing out at the orifice of emission at E, in 143 seconds. 2. By 13 pints, in 133 seconds. 3. By 12½ pints, in 125 seconds. By 12 pints. 5. By 11 pits, in 95 seconds. 6. By 14 pints, in 114 seconds. 7. By 12 pints, in 102 seconds. 8. By 12 pints, in 108 seconds.

9. The orifice of emission being half an inch in diameter, the bottle was filled by 12 pints. 10. By 13 pints, in 133 seconds. 11. By 12¼ pints.

12, 13. The fall being 41 inches, the orifice of emission four lines in diameter, 11 pints filled the bottle in 95 seconds; and 14 pints, in 120 seconds.

With an height of 18 feet and fall 50 inches, 20 parts of water carried down one of condensed air.

The difference in the time, and the quantity of effluent water required to fill the bottle with air, in these trials, was probably occasioned by a portion of the air being sometimes contained in the higher, and at other times in the lower part of the pipe A B, at the commencement of the effusion: or, perhaps,

perhaps, in part, by the water in the ciſtern not being always of the ſame height; for the ciſtern did not overflow, but was ſupplied with great care, ſometimes by a pump, and ſometimes by letting water out of a veſſel, always keeping the ſupply from agitating the contents of the ciſtern as much as poſſible. Had the bottle been larger, there had probably been more uniformity in the reſults of the trials. In eſtimation, I think, however, we ſhall not overrate the operation of this machine, by taking 13 pints for the mean quantity of effluent water emitted while the bottle was filling with air; and then deducting the quantity expelled from the bottle, it will appear that 20 parts of water carried one of air down the pipe A B: and as one ounce meaſure of condenſed air at leaſt was collected in 14 ſeconds, ſo 16 pints would collect every hour.

Trial with a greater length of ſyphon:

Some few days after theſe experiments, the pipe A B was lengthened to 24 feet 7 inches, and D E to 20 feet; but upon trial, the air was carried into the bottle ſo much ſlower than before, that a ſuſpicion aroſe that ſome part of the apparatus was not air-tight; and on this ſuppoſition the pipes were taken down.

Leſs air was carried down.

In February 1804, the pipes, &c. were examined, and ſet up again with conſiderable care. A B was 24 feet 7 inches long; D E, 21 feet one inch; conſequently the difference for the fall was 3 feet 6 inches. With this apparatus, when the diameter of the higher orifice of the pipe D E was four lines, it appeared by four trials (Feb. 25, 1804), that the bottle loſt only one ounce of water per minute.

When the pipe D E was ſhortened to 19 feet 7 inches, and had its higher orifice five feet below the ſurface of the water in the ciſtern, four ounce meaſures of condenſed air deſcended into the bottle, during the emiſſion of 16 pints of water, through the orifice at E, when half an inch in diameter.

The diminution in the collection of air, in theſe laſt experiments, was much more conſiderable than was expected to happen, either from the abſorption of the water, or the increaſed condenſation of the air, which might be occaſioned by ſo ſmall an addition being made to the apparatus. The jet at E was projected more ſteadily in theſe laſt, than in the preceding trials; and the condenſed air, inſtead of riſing into the bottle in large detached bubbles, aſcended in a continual ſtream, like the evolution of gas from the bottom of an efferveſcing

mixture. From the minute division of the air, it is not improbable, that a small portion might be carried along with the current of water under the bottle, and ascend through the pipe DE; but this was not determined. If there was no defect in the apparatus, it appears, that it will not be so advantageous, in the construction of a working machine upon this principle, to employ a condensing column so heavy as 24 feet, as one that is lighter.

Other trials. Feb. 28, 1804. The pipe A B being shortened to 22 feet 5 inches, and D E to 18 feet 3 inches, the orifice of the adjutage at E being four lines in diameter, the bottle was emptied by 13, 12½, 14, 13, 13, 12, and 14 pints of effluent water, in seven successive trials, as in those of May, 1803.

March 9. With a fall 3 feet 9 inches, A B being 15 feet one inch, half a pint of air was collected during the discharge of 12½ pints of water. Again, the orifice of emission being four lines in diameter; the fall 4 feet 2 inches; A B 15 feet 8 inches; D E 11 feet 6 inches; nine ounce measures of air were conveyed into the bottle, in one minute, during the discharge of 10 pints of water, in five successive trials: and when the diameter of the higher orifice of the pipe D E was half an inch, the same quantity of air was carried down in 50 seconds, by nine pints of effluent water, including that displaced from the bottle. When A B was 15 feet 8 inches; D E 13 feet 2 inches; the fall 2 feet 6 inches; nine ounce measures of air were collected in the bottle, by the discharge of 16 pints, in 90 seconds; in 105 seconds, by 18 pints; and again, by 18 pints, in 90 seconds.

March 16. The fall being 2 feet; A B 8 feet 5 inches; D E 6 feet 5 inches; and the diameter of its higher orifice half an inch; ten ounce measures of air were collected in one minute; again in 64 seconds, when the effluent water measured 10 pints; and again in one minute, when 10 pints.

Having now shewn what power a machine operating upon this principle may be expected to possess, I proceed to shew how its principle may be applied to practice.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon. *Fig.* 2, *Plate I.* exhibits a machine for raising water above the cistern.

R represents a cistern supplied by a spring, where there are four or five feet fall for the water.

W W.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

W W, a well or pit situated below the bed or lower level of the streamlet; its depth varying from 6 to 20 feet, according to the elevation to which water is to be raised above the cistern, and the number of progressive columns by which it is to ascend.

A A, a pipe leading from the cistern R, to a bell-shaped vessel B, fixed a little above the bottom of the well, with its mouth downwards. The top of the pipe is crooked, as represented at A, *Fig.* 5, and there is a joint below, which allows the crooked part to be detached from the rest. The lower end of this pipe is also crooked, and turns under the side of the vessel B.

C C, a pipe fixed into the top of the vessel B, and carried a little above the cistern, where two smaller pipes, E and G, are connected with it by a stop-cock.

E, a small pipe leading to F, a vessel or chamber, placed in the cistern R.

G, another small pipe leading to H, a vessel or chamber, somewhat less than F, placed in a higher situation. This pipe has a turn, a foot above the top of the vessel H.

I, a pipe leading from the cistern R to the vessel F.

K K, a pipe descending a foot or more below the vessel F, and then ascending to the vessel H.

L, a pipe connected to K, a foot below H, and then carried to the conduit or cistern which receives the raised water,

The pipes I, K, L, have each a valve opening upwards.

The construction of the cock is represented in *Fig.* 3. The conical barrel has four holes, C, E, G, O, and the turning part or key, has a notch, or hollow, on each side in that part which moves opposite those holes, so that the pipes C and E, or C and G, may be connected by a quarter of a turn. When the communication opens between C and E, the external air has access to the inside of the pipe G, and to the chamber H above, through the opening O; and when C is joined to G, the air is admitted into the chamber F, through the opening O, and the pipe E.

In the narrowest part of the vessel B, a buoy or float is fixed upon the elevated end of a crooked lever, moving upon a horizontal axis, supported by pieces attached to the side of the vessel. Instead, however, of a common hollow buoy, it will be preferable to use a body specifically heavier than water, and by

Defcription of the machine as conftructed for raifing water, by air condenfed in an inverted fyphon.

by adding weight to the other end of the lever, to make fuch adjuftment, that while both are immerfed in water, the body within the veffel B, fhall be a few ounces lighter; and, on the contrary, when it alone is above the water, it fhall be fo much heavier than its counterpoife, which is covered. A fmall cylindrical veffel $2\frac{1}{2}$ inches in diameter, and the fame in depth, filled with water and clofed, will probably be fize fufficient for fuch a float; and the proper counterpoife may be very readily and eafily found, by having the lever fixed on its axis in a veffel of water, repeatedly drawing off and replacing the fluid in that part of the veffel which contains the float; and increafing or diminifhing the weight, until the proper adjuftment is obtained. But to proceed with our defcription.

S reprefents a fmall fyphon fufpended by a lever, with one branch in the infide, and the other on the outfide of the ciftern R. The outfide branch being re-curved in the manner reprefented in *Fig.* 5, it is evident, that when the inftrument is filled, it will draw water out of the ciftern whenever the orifice of the re-curved branch is depreffed below the level of the water in the ciftern; and that its operation will be fufpended by raifing the fame a very little above that level. From the contrary end of the lever, a chain or wire defcends, and is connected to the lever which carries the float; and by this connection, the fyphon is fufpended with the orifice of its re-curved branch above the furface of the water in the ciftern, while the float occupies the higheft part of the veffel B; for the weight of the fyphon and that of the included column muft be fo nearly counterpoifed by the chain and an additional weight, that it cannot deprefs the float, though it muft poffefs fufficient weight to defcend when allowed by the defcent of the float.

M reprefents two cuneiform buckets, connected at their bafes by a tranfverfe partition, and fixed upon a horizontal axis, as is more clearly exhibited by the fection, *Fig.* 4. When the bottoms of thefe are placed in an oblique direction, making an angle with the horizon of 25 or 30 degrees, as reprefented in the drawing, (*Fig.* 2.) and a fmall ftream of water falls from the fyphon, the higher bucket will receive the water, and falling in confequence of its load, will raife the other bucket, which will now receive the water, and by falling will raife the firft, whofe contents were emptied in its defcent.

descent. A small arm or lever is fixed to the axis of this apparatus, and connected by a piece of strong wire to another shorter lever fixed upon the smaller end of the turning part of the cock C, and by this means the cock moves with the alternate motion of the buckets M, when supplied with water.

Description of the machine as constructed for raising water, by air condense in an inverted syphon.

When the space from the top of the vessel B, to the surface of the well is equal to 18 feet, the top of the chamber H, above the bottom of F, and the perpendicular height of the pipe L, above the bottom of the chamber K, may be each 18 feet.

The valves and every other part of the apparatus being in proper order for work; the well being filled with water; and the reservoir R constantly supplied, so as to overflow in one part somewhat lower than the rest of the brim, while the higher orifice of the pipe A A, is about two lines below the surface of the water, and takes in its full quantity; the manner of bringing this machine into action, and its operation afterwards, may be understood by attending to the following directions, and statement of particulars:

Open the pipe E which leads to the chamber F, by turning the cock C, and water will descend into F from the cistern R, by the pipe I: When the chamber is full, place the two connected buckets M, in a horizontal position, and the cock C, if properly constructed and connected with these buckets, will cut off all communication between the pipe C C and the pipes E G. The air carried down the pipe A A, by the column of water which descends and keeps the well constantly overflowing, will ascend into, and gradually expel a quantity of water from the pipe C C, and afterwards that contained in the higher part of the vessel B also. The float receiving an accession of weight by being out of water, will descend and let down the re-curved syphon S, which will pour water upon the buckets below. At this period depress that bucket which by descending opens a communication between the pipes C and E, and no farther attendance will be requisite.

The pressure of the column in the well, above 18 feet high, being thrown upon the water in the chamber F, by the intervention of the condensed air in the pipe C, the valve in the pipe I will be shut, and water will ascend from the chamber F through the pipe K K into H; the syphon S will also ascend into its place, and cease to draw water from the cistern.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

cistern. An equilibrium being soon established between the acting column in the well, and the re-acting one included in the pipe K K and the chambers F, H, by the expulsion of the air from B, the water will afterwards ascend much slower into H than at first, an equal depression of the fluid being produced in the pipe C and the chamber F, by the collecting of the condensed air.

In more or less time, according to the capacities of the chambers, so much water will be expelled from the lower as will fill the higher, the air having been expelled from this last, through the pipe G, whose outlet is at O in the cock C E G O, *Fig.* 3. Water will then begin to ascend into that part of the pipe G which rises from the top of the vessel, and the acting column being lengthened in proportion to the increasing height of that which it counterpoises, the condensed air will depress the water in the vessel B below the float, which descending, will lower the re-curved syphon, and water will fall into the elevated bucket M, which soon afterwards in consequence of its load will descend, and by moving the cock above, will open a communication between the pipes C and G, and between the inside of the chamber F and the external air, when the condensed air will rush out, and this vessel refill with water from the cistern.

The force of the acting column being now thrown upon the contents of the higher chamber H, the valve in the pipe K will close, and water will ascend through the pipe L, into the cistern appropriated for its reception.

At the first opening of the communication between B and H, the re-curved syphon S will pour water into the bucket last elevated; but before the load is sufficient to move the apparatus, the syphon, if properly adapted to its purpose, will be drawn up again, in consequence of the condensed air being expelled faster from the vessel B, than it descends by the pipe A A: for the water will always rise with the greatest rapidity after the turning of the cock, because of the difference subsisting between the acting and re-acting columns, and the air previously stored up in the vessel B. The supply of condensed air, however, being inadequate to support the difference, an equilibrium soon takes place, by the water ascending into the lowest part of the pipe C.

The

The ſupply of condenſed air will now continue to force water out of the chamber H, and to depreſs the fluid in the pipe C, in an equal degree, until all the water in the chamber is expelled. The water will then ſink in the higheſt part of the pipe K, and the acting and re-acting columns proportionally lengthening, air will collect in the veſſel B; the float and re-curved ſyphon will deſcend; and before the depreſſion of water in the pipe K reaches the lower end of the pipe L; before the condenſed air can eſcape by expelling the water contained in this pipe, the elevated bucket M will fall in conſequence of water poured into it by the ſyphon, and the communication between C and E being opened, the condenſed air will ruſh out of H, through the open pipe G, whoſe outlet is at O in the cock C E G O, *Fig.* 3; the valve in L will ſupport the water above it, and the force of the acting column being again thrown upon the water in the chamber F, the fluid will begin to aſcend into K; the re-curved ſyphon will riſe and continue in its place as before, until the water begins to fill that part of the pipe G which riſes from the top of the chamber H, or, if there cannot be a ſufficient quantity raiſed from the lower chamber to fill the higher, until it begins to be depreſſed into that part of the pipe K, connected with the bottom of the chamber F; the ſyphon will then be let down by the float, will pour water into the bucket laſt elevated, and thus again open the communication between the air-holder B C, and the higher chamber, from which the water will be expelled; and in this manner the alternations will proceed, the machine continually raiſing water from one or other of its chambers.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

By additional pipes and chambers, ſimilar to G H L, a ſmaller quantity of water may be raiſed ſtill higher, the loweſt additional chamber being fixed upon the top of the pipe L, and the pipe for ſupplying it with condenſed air, connected to the pipe E. A pipe for ſupplying a chamber ſtill higher muſt be attached to the higheſt part of the pipe G; and it is evident, that if E leads to two chambers and G only to one, the machine will regulate its operations ſo as to loſe no time. It will be requiſite, however, to fill the additional chambers with water before the machine is ſet to work.

Where the ſupply of water is variable, the machine may be adapted by having ſeveral pipes ſimilar to A A, but ſome wider

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

wider and others smaller, and by setting such of these to work as are requisite, for whatever may be the increase or diminution of power, the turning of the cock will be duly regulated by the float.

The water which supplies the pipe I, and ascends in the apparatus, should be cleared, by filtration, from impurities and substances that would obstruct the closure of the valves. The cistern should have a moveable piece at the place where the water overflows, to accommodate the surface of the fluid to the ends of the pipes, that the full quantity of air may descend with each column, and that the maximum of effect may be obtained from the supply.

To determine how well such a combination as that I have described would answer the purpose, I had a model constructed upon the plan exhibited in *Plate I. Fig.* 2; but having no convenience for an overflowing well or cistern, I was obliged to modify some parts, in a manner tending to diminish its power.

The water from the cistern R, falls into a capacious vessel, from whence, when the machine is at work, a hand-pump continually raises it again into a vessel above, which supplies the cistern R through a pipe nearly half an inch in diameter, under a constant pressure of 3½ inches charge. The supply keeps the cistern continually overflowing, and the surface of the water is calm and always at the same height.

The pipe A A is 8 feet 3 inches in length, and half an inch in diameter. Its lower end is inserted into the vessel B, which is closed at bottom, and constructed of such a form as to include the lever which carries the float. Above that end of the lever bearing the counterpoising weight, a vertical pipe 6 feet 3 inches, is connected to the top of the vessel; and through this pipe, which is no wider than A, the water ascends and flows to the pump: A chain, consisting of pieces of wire four or five inches in length, looped together by the ends, also passes through it, and connects the float-lever to that which moves the re-curved syphon S.

The bottoms of the buckets M are both together 14 inches long and 6 broad. The base partition is 4 inches high. Each bucket has an end parallel with the base, one inch deep, provided with a hole to let out the water when depressed. The cock moves with less than a pint of water in the elevated bucket.

The

The float in the vessel B, is a small cylindrical copper vessel, one inch in height and two inches in diameter. It was filled with water and closed before it was fixed in its place. Though equal in bulk only to one ounce and three quarters of water, yet it is quite sufficient to move the syphon which would work a larger machine.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

The pipe C is half an inch in diameter: E, K, G, L, are smaller. The valves are leather.

This machine, when in good order, raises water nearly 12 feet above the cistern, at the rate of $20\frac{1}{2}$ pints per hour, and performs all its operations as well as can be desired. When first set to work, the cock is so placed as to close the top of the pipe C, until the condensed air begins to collect in the vessel B, and then the communication is opened to the chamber F. If the communication was open at first, the water would be expelled from F into the cistern, while the pressure was insufficient to close the valve in the pipe I.

The chambers F H being small, the syphon moves frequently; but in a working machine these vessels should not only be broad and shallow, but capacious, that the wear of machinery may be reduced to its utmost extent.

To determine what quantity of water flows through the apparatus, I fixed a spout upon the top of the ascending water pipe; but in doing this I entangled the float in the vessel B, that it could not be made to work the syphon. The effluent water, in this unemployed state of the machine, including half a pint displaced from the vessel B, amounted to $8\frac{1}{2}$ pints. The superfluous water from the cistern R measured $8\frac{1}{2}$ pints also. If the machine had been working the waste water would have been less, as part would have been drawn off by the syphon.

From several trials, this model appears to raise water above the rate that might be estimated by the experiment of March 6, previously related.

In actual practice, I think the allowance for waste and working the buckets, of one third or perhaps only of one fourth of the supply, will be sufficient; then supposing the apparatus so adapted to the supply, as 28 or 30 gallons from the cistern will raise one gallon 18 feet, so 84 or 90 gallons will raise one gallon 44 feet, by three ascending columns.

The

The bucket-engine at Irton-Hall, in Cumberland*, is said to raise one gallon of water 60 feet high by 36 gallons supply; hence, if the waste water be included, it appears that our machine will not be competent to perform half as much work by the same supply, and its peculiar advantages must depend upon its durability when constantly employed.

I am, Sir,

Your's respectfully,

WILLIAM CLOSE.

V.

Remarks on COUNT RUMFORD's *Experiments relating to the Maximum Density of Water. In a Letter from Mr.* JOHN DALTON.

To Mr. NICHOLSON:

SIR,

Count Rumford's experiments on the max. density of water considered.

IN your last Number, page 225, is an interesting article on the question, At what point of temperature water is of greatest density? From the introductory paragraph I was led to expect, that all the material objections to the current doctrine were considered and obviated, and that new and convincing arguments in its support would be adduced. In the former of these expectations I was altogether disappointed; and though the new experiments are ingenious and well worth attention, they are not quite so demonstrative to me as they appear to be to Count Rumford. Perhaps we may both be too strongly biassed towards preconceived theories: however this may be, it seems proper that when new facts are brought forward, we ought to reconcile them to the theory espoused.

Mr. Dalton's exper. in this Journal on the same subject.

At page 93 of Vol. X. of this Journal, I have stated a number of facts and experiments which appear to me irreconcileable with the notion of water being densest at 40°. I believe it is densest at 32°, or the freezing point; and it is my present intention to shew how, on my hypothesis, I explain Count R.'s results.

* See Philos. Journal, Vol. II. p. 60.

Water

Water expands by heat from some point (whatever it may prove to be) by a law which is nearly that of the square of the temperature from the said point, as is evident from Sir Charles Blagden's table. Consequently the force of ascent, which water acquires by temperature, is at first very small, but increases to a very considerable amount before ebullition. The cohesion of the particles of water is a constant force; there will therefore be a point of equilibrium between these two forces; that is, a point at which the increased temperature will be but just sufficient to counteract the tenacity, in which case no internal motion can ensue. Whether a diminution in density in water to the amount of *one hundredth* or *one thousandth*, or *one ten thousandth* or more, is the point alluded to, is to be determined only by experiment. I apprehend that water of 40° is about one *ten thousandth* part lighter than water of 32°; but that this force of ascent is but just sufficient to counteract the tenacity, and consequently no motion takes place; in such case the diffusion of heat through water is the same as through a solid body. Whenever the difference in density exceeds that just mentioned, internal motion is the consequence, and that greater in proportion to the difference of density, which we know may amount to $\frac{1}{21}$ of the whole.

Count Rumford's experiments explained, on Mr. Dalton's hypothesis. Water has very little change of dimensions, by heat or cold near its max. density.

This change at 40° is not sufficient to produce a current;

Count R.'s experiments therefore will be explained by observing that the thermometer acquired heat by the proper conducting power of water, as if it had been metal, or any other solid body; the temperature acquired was greater in the 2d experiment than in the 1st, because the heat of the ball was greater; but in the 3d experiment the heat of the ball was such as to produce a current upwards that almost precluded the descent of heat, by carrying away the heated particles as soon as formed.

—and therefore the heat at first passed downwards as if thro' a solid; but in higher temperatures there was a current, which prevented the descent.

The circumstances of the two thermometers by the side of the ball and the cup, in the two first experiments not acquiring any temperature, is certainly remarkable, and not easy to be explained, even upon Count R.'s principle; for, the supposed descending current of warm water should have filled the cup and overflowed, so as to affect the collateral thermometer.

Difficulty respecting the two thermometers.

One most important experiment Count R. has omitted, and which it is particularly desirable that he, or some one in possession of a similar apparatus, would perform, especially as it would go further than any other to establish the doctrine of cur-

Important experiment proposed. Let the water be at 40° and the solid at 32°.

rents

rents in water, when the temperature varies from 32 to 40°. This is to repeat the 1ſt experiment, with the difference that the maſs of water ſhould be at the temperature 40°, and the ball at 32°; in which caſe the thermometer in the cup would not be at all affected upon Count R.'s principle; but if the explanation I have attempted above be accurate, no material difference in the reſults of the two experiments would be obſerved.

I am your's,

J. DALTON.

Mancheſter, Auguſt 17, 1805.

VI.

On the Art of bending Wood. By J. H. HASSENFRATZ.*

Either live or dead wood may be bended.

THE operation of bending may be performed either on live or dead wood, the proceſſes differing however for theſe two ſtates.

I. *On the bending of Live Wood.*

Live wood may be

Live wood has a natural elaſticity †, which varies according to its ſpecies, ſize, and age. The larger and older the wood, the leſs elaſticity it poſſeſſes.

—bended for ſhip-building, or for the felloes of wheels, &c.

This operation is performed on wood when growing, either to ſtraighten it, to give it a figure ſuitable to the ornamental purpoſe for which it is deſigned, or to ſhape it to the uſe for which the timber is intended when cut. Thus trees may be bended, which are intended for the building of ſhips, or for making the felloes of wheels in one piece.

By faſtening down young trees.

When trees are yet young and pliable, their ſtems are faſtened down by ropes, or poles, or ſtakes, or frames. In this ſituation they are confined till they will retain when let looſe the curvature that has been given them.

This the moſt eaſy proceſs;

Of all the modes of bending timber the moſt eaſy and commodious is that applied to young growing trees: for their pli-

* Tranſlated from the Journal des Mines, N. 94, p. 475, July, 1804.

† In many parts of this paper the writer ſeems to have uſed the word elaſticité for the property of undergoing flexure without breaking. T.

antneſs

antnefs and elafticity enable them to acquire any form that may be defired; fo that there are few to which the moft whimfical figures may not be given, with due care and the requifite precautions; but at the fame time we injure their natural conftitution, retard their progrefs, and frequently reduce them to a ftate of conftraint and difeafe prejudicial to their growth. but injurious to the timber.

II. *Of the bending of Dead Wood.*

The bending of wood that is cut down and dead, though more difficult, is yet more in ufe, becaufe we may choofe fuch as is beft adapted to the purpofe for which it is defigned, and then give it the fuitable curvature. The bending of dead wood is moft advantageous.

The procefs generally employed is founded on the property caloric poffeffes of augmenting the elafticity of wood by penetrating it, and diminifhing this elafticity on quitting it. The principle is the action of heat, which increafes the pliablenefs of timber.

Thus when we wifh to bend thin pieces, as the ftaves of barrels, or the planks of boats, we heat them at the part that is to be curved, and bend them gradually as they grow hot.

But heat applied to one part of the wood, while the other is in contact with the air, heats it unequally, and increafes the pliablenefs but partially; fo that on bending it, fome parts are ftiff and others yield, occafioning an unequal curvature, and fometimes cracks or fplinters in the infide or on the furface of the wood. The only method of remedying this inequality is to heat the wood equally throughout. Partial heat affects the wood unequally, and occafions it to crack or fplinter.

Furnaces or ftoves gradually heated are adapted to the purpofe of affording a uniform heat, and confequently facilitating the curvature of the wood; but in ufing them there is reafon to fear, that the caloric, while heating the wood, may expel from it the fluids contained in it, char it, and wholly deftroy its elafticity. Furnaces or ftoves heat it uniformly, but may fcorch it.

The pliablenefs of wood is in proportion not to its temperature alone, but to its humidity likewife. The fame wood at the fame temperature will be more or lefs pliable in proportion to the water contained in it; and at an equal degree of moifture its elafticity will be proportional to its temperature. Humidity as well as heat neceffary to render wood pliable.

We have an inftance of the double influence of heat and moifture in joining two pieces of wood with a tenon and mortife, where the mortife is only a third of the breadth of the piece that is driven into it to form the joint. Thefe joints, fo extraordinary in appearance, furprife people fo much, that moft The fingular ftructure of a mortife and tenon.

Heat and moisture are applied to bend timber; most of those who use them make a mystery of them. The process employed in this operation has given rise to the method at present in use for bending with ease the largest and stiffest timber: it consists in penetrating it with humidity, and at the same time imparting to it a uniform temperature, then bending it, and letting it cool, while it is kept in the form to which it has been brought.

—in three different ways: For heating and moistening the timber, three different processes have been employed: first, boiling water; secondly, steam; the third, wet sand heated.

1st. By boiling water. The stove for the first process consists of a large copper boiler, heated by three furnaces, closed by a movable cover, and varying in its dimensions according to the size of the timber for which it is intended. Cranes are used for raising the timber, and putting it in or taking it out of the boiler, which is kept full of water. When the timber is in, the cover is put on and beaten down close, to diminish the evaporation of the water; the three fires make the water boil, the timber is heated and penetrated with moisture, and it is then taken out to be bent.

This dissolves some of the component parts, and lessens the dimensions of the wood. This process, one of the first that was employed, has the defect of dissolving a part of the proper substance of the wood in the boiling water; the timber shrinks in drying, so as to become narrower and shorter; its strength and elasticity are considerably diminished, and from these alterations occasioned by it the process is disused.

2d. By steam. Description of the steamer. Figures 2, 3, and 4, Pl. II. represent the plan and elevations of a steamer. It consists of a large wooden box, formed of stout planks, held firmly together by square frames. Within are supports for the timber that is to be exposed to the action of the steam. The dimensions of the box are regulated by the size and quantity of the wood intended to be softened.

For small steamers a boiler is fixed at one extremity of the wooden box, and the wood is introduced at the other through an opening, the door of which either slides in a groove or turns on hinges. For large ones the boiler is fixed in the centre, and there is an opening for the timber at each end. In the side opposite the boilers are openings *a a a* for arranging the timber on the supports. It is usual to leave the wooden boxes exposed to the air externally; but it would be of advantage to cover

It should be covered with some bad conductor of heat.

cover the planks with some substance that is a bad conductor of heat, to confine the heat that is disengaged from the steam within.

Each boiler having a communication with the interior of the box, by means of a pipe, the steam is distributed to each stage by the tubes *b b b*, *Fig*. 3. The vapour arising from the boiling water penetrates the timber with moisture, heats it, increases its elasticity, and renders it fit to be bent.

Steamers require little care, and little expense, but they cannot be used for timber of any great thickness, since they cannot impart a emperature higher than that of boiling water, and this is not sufficient to give large pieces the degree of pliableness necessary for bending them.

This process is easy in use, and not expensive; but it is not hot enough for large timber.

This lowness of temperature gave rise to the invention of the sand-stove, which is formed of four stone or brick walls. In the middle are two furnaces, with which several circular flues communicate, for conveying the heat, the heated air, and the smoke, to a chimney rising from each end. On these flues are plates of cast iron, which form the bottom of the cavity in which the sand is placed; the flame and smoke circulating in the flues heat these plates, and these plates heat the sand. This is an imitation of those sand-baths which have been long employed in many chemical processes and in several manufactories.

3d. By wet sand. Sand stove described.

As the sand may be heated to a temperature above that of boiling water, it can communicate a greater heat to the timber; but were there nothing but sand and timber in the stove, all the gasefiable substances in the timber might be expelled by the heat, and the timber charred.

To prevent this, one or two boilers filled with water are placed in the middle of the stove. The water converted into steam by boiling penetrates the sand with moisture; this imparts moisture to the timber; and thus the heat that pervades the timber expels from it no more moisture than is replaced by the sand, so that all the proper substances of the timber are preserved.

Steam must be used with the sand heat;

We will not venture to affirm however, that no portion of the component parts of the wood is evaporated in this operation, and that consequently it undergoes no alteration; but with the precaution of taking out the wood to bend it as soon as it is sufficiently heated and penetrated with moisture, the injury is imperceptible.

and the timber will not be perceptibly injured.

The

The sand-stove is covered throughout its whole length, to retard the evaporation of the moisture contained in it, and allow the heat to accumulate sufficiently to give the wood the proper temperature.

Manipulation for the heat.

The pieces of timber are introduced at the ends, placed in the middle of the stove in the direction of its length on bars fixed for the purpose, and covered with sand.

When the timber has been heated and penetrated with moisture to the proper degree for enabling it to assume the degree of curvature required, it is bent to a line designating the curve.

The wood may be bended horizontally or vertically,

The timber may be bent in two ways, either horizontally or vertically; the former is used for pieces of smaller dimensions and greater curvature.

—by any mechanic power;

In either way the force that produces the curve is applied by means of ropes, tackles, or even capstans. The piece must be kept in the shape to which it is brought, and thus left to dry and grow cold, when it will retain the curvature given to it.

—which may be applied various ways.

Frequently when the piece of wood is thin, pressure by hand, or by weights, will bend it sufficiently, so that it will retain its shape on cooling. But the means of bending it may be varied to infinity, according to the elasticity of the timber, its size, its temperature, and its humidity.

VII.

Experiments made in the great, in a reverberating Furnace on Cast Iron, confirming the established Theory respecting the Difference between cast and malleable Iron. By G. A. LAMPADIUS, *Prof. of Chemistry and Metallurgy at Freyberg.**

The reverberatory furnace described.

I SHALL first describe the reverberatory furnace used in these experiments. It had three principal parts: 1. the air tunnel

* Extracted by J. F. Daubrusson, in the J. des Mines, from the *Sammlung Practisch-chemischer Abhandlungen* 'Practical Chemical Essays,' of Lampadius, Vol. II. p. 145.

Prize question of Bohemian Society, 1795.

In 1795, the Royal Society of Bohemia proposed as a prize question to settle the theory of the refining of iron, taking as a basis the labours of Vandermonde, Berthollet, and Monge, on the different states

tunnel and ash-hole; 2. the fire-place; 3. the hearth and chimney. To obtain the proper degree of heat, the air was conducted through a vertical tunnel several ells long (the Saxon ell is near two English feet), the lower aperture of which was over a stream of water, and consequently it brought rapidly to the fire-place a supply of fresh and condensed air. The fuel was wood; the bottom of the furnace was an oval cavity, covered with a heavy coating, and capable of containing three or four hundred weight of metal. The flame, which traversed the furnace with rapidity, escaped afterwards through a chimney eight ells high. The furnace had an opening capable of being closed at pleasure by an iron door. There was another above the fire-place, a few inches square, serving to admit the nozzle of a pair of bellows, or the neck of a retort.

It was an air furnace having an hearth within and its chimney was 16 feet high.

In the use we made of this furnace I had an opportunity of observing very distinctly, that in the flame of a closed reverberatory furnace there are always a multitude of unoxided particles of carbon, which impart to it the capability of reducing (disoxiding) metal. This opinion I had already announced on occasion of a memoir of Mr. Dacamdra. In some of our trials, making use of the wood of the Scotch fir, we observed, that the smoke issuing out was black and dense, and this the more the fresher the wood; but as soon as we made use of the bellows, the flame appeared clear, because the oxigen introduced by the air or vapour oxided the carbon that was in the flame, and thus produced a greater heat.

Unoxided particles of carbon visible in the flame of this closed furnace.

First Experiment with the simple Fire of the Furnace.

Exp. I.

The furnace having been heated for some hours, and the fire being very violent, about three hundred weight of metal was taken from the crucible of the high furnace, and poured into the reverberating furnace. This cast iron, when become solid, was gray, and of a fine grain. At the expiration of an hour a frothy scoria appeared on the surface of the metal, which, to

Gray fine grained cast iron in the reverberatory furnace, covered with frothy scoria, chiefly carburet of iron.

states of iron. Mr. Lampadius shared the prize. His memoir may be considered in general as a confirmation and supplement to the labours of the French academicans; the experiments which he made at Muckenberg in Saxony, in the iron works of Count Von Einsiedel, affording him fresh proofs of this theory. These experiments are here presented to the reader. D.

judge from appearances, consisted chiefly in carburet of iron. We attempted to remove it; but as some of the metal adhered to it, and came away at the same time, we desisted. Soon after, the furnace being closed, we heard a continual boiling, resembling that of a viscous substance in a close vessel. On opening the furnace, we perceived that the whole matter in reality boiled, and that bubbles were continually rising, which burst on the surface with a beautiful bluish flame. These jets of flame had the colour exhibited by carburated hidrogen gas. The boiling continued as long as the fire was kept up; at the same time a pretty large quantity of scoria was formed, which however could not be removed, on account of the viscous consistence now acquired by the metal. Besides, as the metal was frequently stirred to present a fresh surface to the air, the scoria mingled with it. At the end of five hours it was no longer fluid, and appeared to be refined. It had lost its gray colour and fineness of grain, was white and coarse grained, and showed itself more malleable, though it was not capable of being forged. The refiner carried it to his ordinary furnace, and there it was refined in less time, and required less labour than common cast iron.

Not removable for adhering metal.

The metal was brought to ebullition.

Carburated hidrogen gas evolved.

In five hours it became white and coarse grained,

and a little malleable.

This was afterwards refined sooner than common cast iron.

As in this trial we were unable to separate the scoria, and no change had been made in the form of the hearth of the common refinery, which ought perhaps to have been done, nothing positive can be advanced with respect to the practical advantage of refining by the help of reverberatory furnaces; we were merely convinced of its possibility, and enabled to demonstrate the theory of this process, that is to say, of seeing clearly what passed in the operation. The cast iron was here converted into malleable by means of the oxigen that was in the little atmospheric air, which, jointly with azote and carbonic acid gas, covered the metal in fusion. This oxigen combined with the carburet of iron, and then carbonic acid gas and oxide of iron were formed; this produced the bubbles of air and the scoria. The lightness of the frothy scoria, which arose to the surface at the beginning, was the reason of their separation from the rest of the mass; but as soon as the air began to act they were destroyed.

The process of refining iron in a reverberatory furnace

shews that carbon is burned off.

Second

Second Experiment; the Fire of the Furnace being assisted by the Vapour of Water. Exp. II:

I had attempted to decompose carburet of iron in small quantities by the help of water in the state of vapour. By heating the carburet red hot, the water was decomposed, and I obtained carbonic acid gas, hidrogen gas, and oxidé of iron. As the chief difference between cast and malleable iron consists in a certain quantity of carburet of iron contained in the former, and which must be separated to render the iron malleable, I was desirous of trying the effect of water in vapour on cast iron in a reverberatory furnace, principally in order to know how far the refining of iron might be carried on in this way.

The carburet of iron is decomposed by aqueous vapour.

Application of this principle to refining iron.

About three hundred weight of cast iron of the same quality as before, and just taken from the high furnace, were put into the reverberatory furnace as in the preceding experiment; we then took a large tubulated iron retort, put into it nine or ten quarts of water, fitted a gun barrel to its neck, and introduced the end of the gun-barrel into the little opening in the furnace. The water in the retort was made to boil, so that the steam diffused itself with the flame over the melted metal. At the expiration of half an hour all the marks of refining that had been observed before were perceptible; the ebullition was considerable, and the flame that issued from the chimney more bright. Two hours after the commencement of the process, fresh water was put into the retort. In about three hours the metal began to thicken, and at the end of four hours it exhibited the marks of refined iron, and we imagined the operation to be finished. We found the grain of this iron finer, however, than that of the iron operated upon in the preceding experiment, and the mass was full of little blebs.

Steam was introduced,

by which the operation proceeded quickly,

but the iron was of fine grain, full of blebs,

We gave it to the refiner, who treated it like the former; but to our great astonishment we found that it wrought worse in the refinery fire than cast iron the most difficult to refine. It required much more labour, and an hour's time longer.

and was less capable of being refined than before.

Having assayed a specimen in the state in which it came out of the reverberatory furnace, I found it to contain a much larger quantity of oxigen. Experience had already taught me, that half a pound of gray cast iron, treated in a retort with four ounces of charcoal from which all carbonic acid gas had been

There was more oxigen in this than in other kinds of cast iron.

been expelled, gave 32* cubic inches of carbonic acid gas. An equal quantity of white cast iron afforded 165 cubic inches of the same air. Four ounces of the cast iron just taken from the reverberatory furnace, mixed with two ounces of charcoal, yielded 96 inches, or 192 inches to half a pound.

Hence we may infer, that the proportions of oxigen contained in these different kinds of cast iron are,

In iron super-refined by the vapour of water -	192
Common white cast iron - - - - -	165
Gray cast iron - - - - - - -	96

This super-refined iron imbibed oxigen from the decomposed water which destroyed its carburet.

To the iron produced in the experiment just mentioned, I give the epithet super-refined †, because I conceive it to have been formed as follows:—The water in vapour was decomposed, and destroyed the carburet, as atmospheric air does in the ordinary refining; but at the same time this water imparted to the iron so large a quantity of oxigen, that in the refinery it was necessary, not only to separate the scoria, but to disoxide the metal likewise. This experiment farther confirms the property iron possesses of becoming oxided in different degrees.

If this experiment afforded nothing practically beneficial, it has at least thrown some new light on the properties of cast iron.

Exp. III.

Third Experiment; the Fire of the Furnace being assisted by the Action of Bellows.

Bellows applied with the reverberatory furnace.

The same furnace was used, and the place of the retort in the preceding experiment was supplied by a pair of double bellows mounted with leather, 5 feet (4 f. 8 in.) long, 3 (2 f. 10 in.) broad, and 4 (3 f. 9 in.) high at the posterior extremity when open. It was so placed, that the stream of air was parallel to the flame and to the middle of the furnace, and worked at the rate of eight or ten strokes in a minute. We were desirous of seeing how far the air thus assisted would carry the refining; the furnace being managed and filled as before.

The heat was much greater.

At the end of half an hour the heat was perceived to be much greater than in the first and second experiments. The phenomena of the refining already mentioned appeared in succession;

* Probably this is an error of the press in the original: as it does not agree with the proportion assigned in the next paragraph, one of the two must be wrong.

† Or surcharged with oxigen.

ceffion; but inftead of the frothy fcoria obtained in the firft effay, a very fluid ftratum was formed, which diffufed itfelf over the melted metal, and prevented its refining. This fcoria, when grown folid, was of a blackifh brown colour and vitreous fracture. We endeavoured more than once to remove it, but the ftratum was fo thin as to render it impracticable: As foon as one ftratum was removed, another formed. At the expiration of four hours, the metal being ftill very fluid, we began to ftir it, in order to bring its different parts fucceffively into contact with the air; this produced an extraordinary heat in the furnace, combuftion, and fcintillation, refembling that which takes place when iron wire is burnt in oxigen gas. This oxidation always produced frefh fcoria: as foon as we defifted from ftirring, every thing became quiet, and the ftratum of fcoria prevented the oxidation. At length, after three hours longer, making feven in all, during which the melted mafs had frequently been ftirred, it feemed to thicken; perceiving too, that it diminifhed confiderably in quantity, the fire was damped, and the matter left to cool in the furnace. It was afterward weighed, and found to have loft much of its weight. Its extraordinary fracture gave reafon to prefume a high degree of oxidation; for inftead of being gray and granulous, it was compact, and of a filver white. It was interfperfed with a large quantity of fpherical cavities, greater or lefs in fize, which evidently announced the exiftence of an aeriform fluid, that had been extricated during the fufion.

The fcoria melted.

It was blackifh brown, of a vitreous fracture, and not to be removed.

Stirring produced extreme heat, and fcintillating combuftion.

The iron had loft much in weight.

Its fracture compact and filvery.

Porous.

This mafs was too fmall to be refined. Having examined the quantity of oxigen it contained in the fame manner as with the other fpecimens, I found that four ounces yielded 87 cubic inches of oxigen gas, and confequently nine inches lefs than that which had been treated by means of aqueous vapour. Thus probably here too the oxidation was too powerful, and the iron was fuper-refined. As the metal did not become doughy in the courfe of the procefs, it muft have been fuperfaturated with oxigen without paffing through the ftate of malleable iron. The carburet, it is true, muft have been totally deftroyed during the operation, which produced the filvery hue.

There was much lofs, and the remainder acquired much oxigen,

without paffing through the malleable ftate.

The carburet deftroyed.

Remarks

VIII.

Remarks on the burſting of two Muſquet Barrels by a Charge of Gun-powder confined by Sand. W. N.

A thin muſquet barrel burſt.

PART of the barrel of a muſquet of which the internal diameter was ſix and a half tenths of an inch, was corked at one end, and fine ſand to the depth of twelve inches was poured in: upon this was poured two inches of gunpowder and a ſmall tube (of glaſs) was then ſtuck in the gunpowder, and the bore of the tube, which was about one twentieth of an inch diameter, was filled alſo with gunpowder. The length of the tube was ſufficient to reach clearly above the top of the gun-barrel, and all the reſt of the ſpace in the barrel, being about thirteen inches above the charge, was filled with ſand lightly poured in. In this ſtate the barrel was ſet up in one corner of a furnace chimney, and a match ſtuck into the glaſs tube and lighted, afforded ſufficient time for the aſſiſtants to remove out of the direct line of exploſion before the effect could take place.

The diſcharge tore the barrel into ſeveral contorted pieces in the part near the charge; the upper part fell unaltered, and its contents of ſand ran out; the lower part alſo fell down, but neither its ſand nor cork were diſturbed, nor was that portion of the barrel affected.

As the thickneſs of the iron did not exceed one thirtieth of an inch, I was deſirous of repeating the experiment with a ſtronger piece.

A thicker barrel charged with gunpowder and ſand.

A muſquet barrel, $2\frac{1}{2}$ feet long, diameter of bore five tenths of an inch, and thickneſs of metal at the breach full one quarter of an inch, was charged with 278 grains, or a little more than half an ounce troy of gunpowder, which occupied the ſpace of four inches. Upon this charge was poured fine ſand to the depth of twelve inches, weighing 1151 grains, or about $2\frac{2}{5}$ ounces troy, and upon this was lightly preſſed down a ſoft wadding of gauze paper, for the purpoſe of allowing the barrel to be placed horizontally without any ſubſequent diſturbance of its charge. It was ſafely placed in an horizontal poſition and fired at the touch hole by means of a train.

Effect of the exploſion.

The barrel was torn aſunder for the length of eight inches, the part neareſt the breach-pin being opened nearly to flatneſs.

The

The fand remained in the barrel. Its face neareft the blaft was confolidated to a very fmall depth, and I think the mafs had been removed or elfe jammed more clofely together; for the fpace unoccupied between the place where the breach pin had been and the furface of the fand was full nine inches. But as the fand was not immediately noticed, I cannot be fure that none might have been driven or fallen out, during or after the blaft; though I am difpofed to think not.

It muft be remarked, that the powder was a very full charge, and that the fand weighed as much as fix mufket-balls of half an inch diameter. I do not however apprehend that the barrel would have burft with fix balls. Remarks.

The blafting of rocks, the fplitting of logs of wood, and the deftruction of artillery when on the point of being abandoned to the enemy, are the leading purpofes in which the application of fand to confine gunpowder is likely to become ufeful. Ufes.

IX.

Report of a Method of meafuring the initial Velocity of Projectiles difcharged from Fire-arms, both horizontally and with different Elevations, made to the Phyfical and Mathematical Clafs of the National Inftitute by Mr. PRONY, *Dec.* 11, 1803. *Abridged from the Original*.*

IT is not much above fixty years fince experiment began to be applied with fuccefs to the theory of projectiles. Mr. Benj. Robins, who may be efteemed the firft in this career, employed a pendulum to determine the initial velocity of mufket balls, meafuring it by the arc of ofcillation. He likewife meafured the recoil, by fufpending the gun-barrel from the pendulum. Robins firft examined the velocity of projectiles by the pendulum, and the recoil of a gun by fufpending it.

About ten years after, the Chev. d'Arcy publifhed a feries of experiments in the memoirs of the French Academy of Sciences, in which he employed two pendulums, againft one of which the ball was projected, while the other, to which the gun-barrel was fufpended, ferved to meafure the recoil. D'Arcy made fimilar experiments with two pendulums.

* Journal des Mines, No. 92, p. 117, May, 1804.

Dr. Hutton with cannon balls.

Fifteen years after this Dr. Hutton made many experiments at Woolwich with cannon balls by means of the pendulum.

Count Rumford's improvement.

About the year 1778, Count Rumford improved this method of trial, and invented a very simple method of suspending the gun-barrel so that the recoil took place without the axis ceasing to be horizontal.

Dr. Hutton's exp. are the most complete on the subject.

Lastly, Dr. Hutton resumed the subject, and made a number of experiments from 1783 to 1786, with much care, and at great expense, on both kinds of pendulum. These may be considered as forming the most complete and instructive treatise we have on experimental ballistics.

Antoni's description of Mathey's machine.

We have not mentioned the labours of Antoni, but we must not pass over a machine described by him in his essay on gunpowder. This, which he says was invented by a mechanic named Mathey, consists of a horizontal circle, the centre of which is supported by the superior extremity of a vertical axis, and serves as a base to a cylindrical envelope of paper. A rotatory motion is given to this cylinder by means of a cord passing over a leading pulley; and the projectile being discharged horizontally, when the angular velocity of the machine is uniform, in a vertical plane in which the axis is found, pierces the cylinder in two points, the distance of the second of which from the diameter passing through the first measures the arc described by the machine during the passage of the projectile.

Col. Grobert's newly invented machine described.

The machine recently invented by Col. Grobert is constructed as follows:

It consists of two pasteboard disks revolving swiftly at the extremities of an horizontal axis.

A horizontal rotatory axis about 34 dec. (11 feet) long carries at each extremity a pasteboard disk perpendicular to it, and fastened to it so that the whole may turn rapidly without deranging the respective positions of the parts.

A rotatory motion is given to the two disks by means of a weight suspended to the end of a cord, which, after having passed over a pulley ten or twelve yards from the ground, is rolled upon a wheel and axle level with the disks. An endless chain, passing round the wheel and the rotatory axis of the disks, communicates to this axis the motion which the weight in its descent imparts to the wheel.

The advantages this machine possesses over Mathey's consist in the horizontal position of its axis, which admits the utmost degree of firmness and regularity in the position and motion of the

the disks: in the projectile not traversing a cylindrical surface, but two vertical planes, the extent and distance of which may be considerable, and thus give very accurate measures: and its being capable, which no other apparatus is, of measuring the velocities of balls of different sizes projected at different elevations.

The projectile is fired through both disks, and the rotation prevents the second hole from being opposite that on the first pasteboard.

All that is necessary in using this apparatus is to give a uniform and known angular velocity to the disks; and to measure the arc comprised between two planes passing through the axis of the disk, and one of them through the hole in one disk, the other through the hole in its opposite.

Method of using the instrument.

In the trials made, the motion became sensibly uniform, when the weight arrived nearly in the middle of the vertical space it had to traverse, as was found by twice measuring the times of the third and fourth quarters of the descent, and afterwards comparing these times with the corresponding spaces passed through. An excellent stop-watch by Lewis Berthoud, and another by Breguet, were used for this purpose.

The descent of the moving weight becomes uniform.

In most of the experiments the vertical space passed through by the weight was measured by the turns and parts of turns of the cord wound off in a given number of seconds, as in all respects most accurate and commodious.

The space of descent was measured by turns of the cord.

To measure the arc a screen, or pasteboard, was fixed before each disk, a very little distance from it, and the hole in the first disk being brought opposite to the hole in its corresponding screen, a rod carried through the centre of these two holes and of the hole in the other screen which would be opposite them, must pierce the second disk in the plane of the hole in the first; and the arc comprised between this point and the centre of the hole in the farther disk would measure the angle described by the apparatus while the ball was traversing the length of the axis.

Method of measuring the arc between hole and hole.

It is obvious, that the fixed screens, which give the absolute direction of the path of the ball, afford the means of shewing the defect of parallelism, if there be any, between this path and the axis on which the disks revolve.

Two fixed screens are used to shew any defect of parallelism between the path of the ball and the axis.

The gun-barrel was fixed horizontally, parallel to the axis of the disks, and at such a distance, that the concussion of the air by the explosion could not affect the motion of the nearest disk.

The time supposed too short to allow a measurable arc.

One thing may naturally suggest itself, which is, that the time of the ball's passing from one disk to the other, through a space of three or four yards, must be less than $\frac{1}{100}$ of a second; and it is difficult to conceive, that in so short a space the disk could describe an arc capable of being measured.

But it did not prove so.

But this difficulty is easily solved by the fact. When the motion became uniform, the wheel and axle commonly made 0·833 of a turn in a second; and every turn of the wheel produced 7·875 turns of the axis of the disks, which consequently made 6·56 turns in a second. Thus a point on the disk three feet from the axis would move about 41 yards in a second, and in $\frac{1}{100}$ of a second $\frac{41}{100}$ of a yard, or nearly 15 inches, a length more than sufficient for the most accurate measurement.

The fire-arms experimented with.

The experiments were made with a soldier's firelock and a horseman's carbine, the lengths of which in the bore were 3 f. 8 in. and 2 f. 5 in. The balls were accurately weighed, found to be on a medium 382 grains troy, and each was impelled by half its weight of powder.

Formula for calculating the velocity.

The following formula was employed for calculating the velocity of the balls. Putting π for the semiperiphery, when radius is unity $= 3.141$; k for the ratio between the turns made by the wheel and axle and the arbor of the disks; t the time employed by the wheel and axle to make a number of turns n; r the distance of the hole in the second disk from the centre; a the arc described by this hole while the ball passes from one disk to the other; b the distance between the disks; and V the velocity of the ball: we shall have the equation

$$V = \frac{2\pi n}{kt} \cdot \frac{r}{a} b.$$

Mean velocity with a carbine, 1269 f. per sec. with a musket, 1397.

The mean velocity deduced from ten experiments with the carbine was 1269 feet and a half in a second; that from the experiments with the musket, 1397 feet. These being in the ratio of 11 to 10 nearly, it would appear, that the length of the soldiers's firelock might be reduced without much diminishing its range*; but there are other circumstances in a military view, by which the length of the weapon used by the infantry requires to be regulated.

* The differences of the range are much less than those of the velocity. See Dr. Hutton.—T.

The

The commiſſioners made ſome experiments with half charges or with powder only to the quantity of one fourth of the weight of the ball. In theſe the mean velocities were, for the firelock 829 feet, for the carbine 822½. Theſe velocities do not differ ſo much from each other, and conſiderably exceed the half of thoſe given by the full charge, which may be aſcribed chiefly perhaps to the more complete firing of the powder. With ¼ of powder they were 822½ and 829.

The commiſſioners were deſirous likewiſe of making ſome experiments on the reſiſtance of the air to the motion of the ball, the diameter of which was from 15 to 16 millemetres (5·8755 lines to 6·2672.) For this purpoſe the mouth of the gun-barrel, which at firſt was 7 f. 9 in. from the neareſt fixed ſcreen, was removed to the diſtance of 67 f. 9 in. In this ſituation the mean velocity of the muſket-ball was 1127 f. inſtead of 1397, ſo that it was diminiſhed nearly in the ratio of 42 to 34. The experiments of this kind however were few in number. The reſiſtance of the air in 20 yards diminiſhed the velocity nearly one-fifth.

There is no doubt but the dimenſions of Col. Grobert's apparatus may be enlarged, ſo as to adapt it to experiments with cannon balls; though it is not eaſy to ſay without trial what dimenſions would be compatible with accuracy of experiment. The apparatus might be enlarged with advantage.

The Colonel likewiſe propoſes an alteration in it, for meaſuring the velocity of projectiles at different elevations, as far as 45°. The following is his contrivance for this purpoſe. Each of the diſks has a ſeparate axis. The wheel and axle has a wheel at each end, with an endleſs chain, one turning the arbor of one diſk, the other that of the other. Thus the rotatory motion imparted by the deſcending weight is communicated equally to both diſks at the ſame time, the wheels and arbors being made exactly of correſponding dimenſions. The ſtand of the diſk fartheſt from the gun is moveable in a vertical direction, ſo that it may be raiſed to the neceſſary elevation; a few links being added to the endleſs chain for every different height. As the diſk is raiſed indeed, it becomes inclined to the path of the ball; but as the greateſt diminution that can take place in this way is in the ratio of about 7 to 5, a ſufficient field is ſtill left for pointing with preciſion. Mode of adapting it to different elevations.

To

Additional machinery for counting time,

To prevent any miſtake from want of attention in the perſons employed, Col. Grobert has added certain pieces of mechaniſm to his apparatus, by means of which the weight, when it has deſcended to a certain point, puts in motion a ſecond pendulum to count the time, and a ſyſtem of wheels and pinions connected with the wheel and axle to indicate the number of turns made by it. By ſimilar contrivances it diſcharges the gun, and ſtops the pendulum and the counter of the turns at the proper time. Theſe may occaſionally be of uſe, but complicated machinery is always liable to get out of order, and it may be diſpenſed with here, if the obſervers be ever ſo little expert and attentive.

The motion of the diſks does not affect the path of the ball.

It might be ſuſpected, that the motion of the firſt diſk would cauſe ſome deflection of the ball from its true path before it reached the ſecond. To aſcertain this, three ſcreens were fixed at equal diſtances, the ſecond and third being placed before the firſt and ſecond diſks reſpectively. Now it is obvious, that the hole in the third ſcreen would not be in the ſame vertical plane with thoſe made in the firſt and ſecond, if any deviation took place.

Experimental proof.

A ball being fired through the apparatus thus arranged, a plumb line was ſuſpended before the centre of the hole in the firſt ſcreen, and the moſt accurate obſervation could not diſcover any deviation, but that the ſame line cut the centres of all the three holes. This experiment was ſeveral times repeated with the ſame event.

This owing to the velocity.

The fact no doubt is, that the extreme ſhortneſs of the time, (for the ſemidiameter of the ball is not the forty thouſandth part of a ſecond paſſing the diſk) does not allow the diſk ſenſibly to affect the path of the ball; much leſs can the ball have any effect on the motion of the diſk.

It may not be amiſs to obſerve, that the diſtance of the fartheſt ſcreen being about twelve yards only, the inflexions obſerved by Robins in diſtances of a hundred yards were not likely to take place.

Fact

Fact concerning the invisible Emission of Steam into the Air.

W. N.

SOON after Mr. Giddy had mentioned to me the very remarkable and curious facts of which an account is given at page 1 of the present Number, I was engaged in the experiments on the simmering of water related at p. 216 of Vol. X. I then made an experiment which may perhaps in a small degree elucidate those phenomena. A small glass tube was stuck through a cork, and this was then pressed into the neck of the retort in which water was boiling over the lamp. The steam was emitted through this smaller aperture in a visible jet of upwards of a foot in length. But when a candle was held with its flame immediately beneath the end of the tube, the jet became perfectly invisible. To determine whether the water might be decomposed, or the steam simply expanded so far as to be absorbed by the air, or if condensed to form a vapour too thin to be perceived, I suffered the hot invisible current which had passed through the candle, to pass through a larger glass tube. In this case visible steam issued plentifully from the farther end: Hence, I am disposed to judge that the large tube having kept the very hot steam together and cooled it so as to render it visible again, there was little if any decomposition of the water. But at the same time, when we consider the disappearance of the dense smoke in Mr. Giddy's experiment, there seems to be great reason to think that the charcoal was oxigenated and gazified. If so, the products must have been expanded and invisible steam, hidrogen and carbonic acid. By collecting the products in an experiment of this kind, these conjectures will either be verified or refuted. If the former, we shall have the decomposition of water and oxigenation of carbon at a lower temperature than has hitherto been shewn or expected.

Steam was visibly emitted in a current:

It became invisible when it passed above the flame of a candle;

but the steam was not decomposed:

Perhaps some part may have been changed.

Experiments

XI.

Experiments made with the Water blowing Machines of the Iron Works of Poullaouen; by Citizens BEAUNIER *and* GALLOIS, *Mine-Engineers* *.

The experiments made to shew the effects of a blowing machine.

OUR object was to ascertain the differences in density of the air within a blowing machine, under the various circumstances by which it might be affected; and at the same time, we endeavoured to find what may be the most advantageous mode of constructing the machine, to produce the greatest effect with the least expenditure of water.

Former accounts are obscure.

One of the chief causes of the doubts that have arisen respecting the suppression or retaining of certain arrangements in the construction, was the omitting to describe the machines, the experiments with which have been compared. We shall therefore previously notice the principal distinctions that may be made between these machines, from the manner in which their effect is produced.

Two kinds of water blowing engines, as the air is received at top, or from the side.

Dr. Lewis observes, that there are two general methods of causing the air to be conveyed by the water in the blowing machines. In the first, the water receives the air by the summit of the machine; in the second it receives it by lateral apertures: and he lays it down as a principle, that those circumstances, which promote the effect in the one case, are detrimental to it in the other.

General observations of Dr. Lewis. The engine is an upright pipe through which a shower of water and air descended.

He observed further, that if the water be at rest in the funnel of the machine, (see Plate III. Fig. 2.) and afterward have liberty to run off, it carries little or no air with it; that if the water have a gyratory motion in the funnel, it carries down a considerable quantity; and that if it fall from a certain height, so as to have been greatly divided, it carries still more: that if the water flow through a pipe with lateral apertures, it receives air through these apertures, even when its motion is slow: that if the pipe be of equal diameter throughout, the quantity of air thus received is inconsiderable;

Lateral apertures preferable for admitting the air.

but if the diameter be diminished to a certain degree at the part where the apertures are made, the quantity of air is greater than could have been introduced through the funnel

* Translated from the Journal des Mines, No. 91.

without

without any lateral openings to the air: laftly he obferves, that air conveyed downward from the top of the tube, or the funnel, prevents the introduction of the frefh air by the lateral apertures, which in this cafe, inftead of receiving more air, let that which has been already introduced efcape.

Lewis concludes, that the two methods by which air may be made to defcend with a ftream of water, ought not to be united in one machine; and that the machine conftructed with a pipe, a funnel, and apertures to let the air enter around or below the throat, produces the moft powerful effect.

The machine on which we made our experiments was of the conftruction which Dr. Lewis has deemed moft advantageous. See Fig 2.

The machine of Poullaouen defcribed.

The height of the fall, taken from the bottom of the channel that conveys the water to the upper part of the barrel B, is 21 feet 6 inches.

Height of the fall.

The height of the funnel, from the bottom of the fame channel to the throat x, is feven feet. This funnel is of the fhape of a fruftum of an inverted cone, the larger diameter of which is 12 inches, the fmaller four. The remainder of the tube down to the barrel, is a cylinder eight inches in diameter.

Funnel at top of the pipe.

The plank N, 12 or 13 inches wide, is fixed one foot below the head of the barrel. The barrel is fix feet high.

Barrel or air veffel.

The water iffues out of the barrel by the triangular apertures t t t, and is conveyed away to a drain by the channel M, the bottom of which is four feet higher than that of the barrel.

The water flows off beneath;

The air compreffed by the external water, the level of which, as will foon be proved, is from 27 to 30 inches above that of the water in the barrel, efcapes through the tube P, which is a hollow cylinder five inches in diameter.

and the air is conveyed through a pipe at top.

This tube P, called alfo the air-pipe, terminates in a conical nozzle, having an aperture of two inches only.

Immediately below the throat x, are four air-holes y y.

Air-holes in the upright pipe.

This being premifed, we proceed to the inftrument employed by us for determining the denfity of the air in the machines.

It was invented by Citizen Vergnies Bouifchére, proprietor of the iron works at Vic-Deffos, in the ci-devant county

Gage for meafuring the denfity of the air.

county of Foix, and is a particular kind of barometer, to which the name of water anemometer has been given. See Fig. 6.

It is a short barometer gage inserted, the fluid being water.

It is composed, 1st. of a cylinder A; 2d. of a tube *c*, bent twice, the lower extremity of which is slightly conical, and terminates about two inches below the bottom of the cylinder; 3d. of a graduated tube *d* inserted in a vertical position into the cylinder, and reaching below the level *n* of the water contained in it.

The tube *c* being inserted into an auger hole made in the side of the blowing machine, and stopping that hole closely, the internal compressed air communicates with the water contained in the anemometer, presses upon it, and in proportion to its density raises to a less or greater height in the graduated tube.

The cylinder A and the tube *c* are of tin. The lower part of the tube *d* to the height of nine inches is of the same material, to which is fitted a glass tube about 36 inches long.

The cylinder A is four inches high and as many in diameter. The greatest diameter of the curved tube is half an inch, the smallest, at the extremity, a third of an inch. On observing however, that the size of this opening contributed to increase the extent of the oscillations in the graduated tube, we endeavoured to diminish it as much as possible. For this purpose we closed the lower part of the tube *c* with sealing wax, in which we afterward made a very small aperture by passing a heated needle through it.

The tube *d* was divided by a scale of inches, beginning from the surface of the water contained in the cylinder *.

ACCOUNT OF THE EXPERIMENTS.

Experiments with the blowing machine.

1. *Experiments relative to the Expenditure of Water, and the Quantity of Air disengaged.*

The blowing machine No. 1, see the horizontal projection, Fig. 1. to which for the sake of clearness we shall refer our different experiments, served for the trial. It was placed in a T, opposite the machine No. 2, destined for the same purpose.

* The great difference between the diameter of the tube *d*, and that of the cylinder A allows the level *n* to be considered as constant.

pofe. The afflux of water into each was regulated by means of the hatches a and b, and the diftant floodgate Q, placed in the principal channel D. See the plan Fig. 1. Plate .

The anemometer was placed in o, Fig. 2, in the direction of the vertical tube P protracted. The hatch placed in b was let down, fo as to prevent the paffage of the water that way. The hatch placed in a was raifed, and the flow of water regulated by means of the floodgate Q. This flow we varied, till we found the water in the graduated tube raifed as high as could be effected without any other change of circumftance. When we were certain we had attained this point, and that no variation in the quantity of water flowing off took place, we made the following obfervations.

1. The mean depth of the water in C, in the little channel, juft before the T, was 15 inches 6 lines. Obfervations.

2. The mean depth of the water in the great channel, was 18 inches 9 lines.

3. The water rofe to 26 inches in the graduated tube. The ofcillations varied between 25 and 27 inches, but feldom reached the latter height.

4. The velocity of the water in the great channel having been obferved, the following data were obtained.

Examined by means of fimple floaters of paper, on an extent of 24 feet, we had,

1ft. The fpace paffed through in two minutes = 61 feet, 8 inches, 6 lines.

2d. The fpace paffed through in four minutes = 120 feet, 6 inches.

The fame velocity examined with cork floats, fupporting little balls of wax, the weight of which was augmented by bits of lead, fo that they fwam in the middle of the ftream with a gravity little exceeding that of water, we had for a mean of the fpace paffed in two minutes, 63 feet, 7 inches, 4 lines. Method of meafuring the velocity of the water.

If we compare thefe different refults, we fhall find, that the mean velocity of the water may be eftimated at 30 feet, 11 inches, 1 line, a minute: but as it appears to us, that the refults afforded by the cork floats muft approach neareft the truth, we will pay no regard to the quantities before obtained, and eftimate the mean velocity of the water in the greater channel, at 31 feet, 9 inches, 8 lines, a minute.

Consumption of water.

Now the breadth of the channel employed is 3 feet, 6 inches, and we observed, that the current, the velocity of which we have given, is 18 inches 9 lines deep. Hence we may conclude, that the consumption of water by the machine, under the circumstances above mentioned, is 173 cubic feet in a minute, the height of the column of water in the instrument being 26 inches.

Quantity of air emitted.

From the method described in the Hydrodynamics of Bossut, we calculated the quantity of air which this mass of water causes to issue from the machine in a given time. This quantity of air was found to be 7.35 cubic feet in a second, or 441 in minute *.

II. *Experiments on the Effect of the Air-Holes.*

Effect of the air-holes ascertained by experiment.

1. On stopping the four air-holes, the water in the tube of the instrument descended to nine inches, and oscillated very little. The efflux of the water from the machine acquired a velocity sufficient to diminish the depth of the water in the little channel C, Fig. 1 and 2, near the T, six inches.

2. One of the air-holes being opened, the water in the tube oscillated between 22 and 24 inches. The mean = 23 inches.

3. A second air-hole being opened, the mean height of the water in the tube was 25 inches.

4. A third air-hole being opened, the columns of water in the tube rose to its former height of 26 inches.

5. The fourth hole being opened, no perceptible alteration in the instrument took place, which proves, that this hole has no effect on the machine.

III. *Experiments on the Use of Crosses placed at the superior Orifice of the Machine.*

The cross bars in the top of the tube be advantageous.

Some iron-masters are accustomed to place two round bars in the form of a cross at the upper orifice of the funnel of the machine. These they imagine increase the effect of the machine by dividing the water at the moment of its fall.

Cylindrical bellows of Namur give more air with less water.

* If these results be compared with those of the cylindrical bellows of the country of Namur, described by Cit. Bailbet, in No. 16, of the Journal des Mines, it will appear, that, to give out an equal quantity of air, the quantity of water expended by the blowing machines, with a fall more than twice its height, is nearly double that employed to move the cylindrical bellows.

To

To judge of this in the cafe before us, we fitted in one of thefe croffes, all the other circumftances remaining as above, and then obferved the progrefs of the inftrument.

They diminifh the effect.

The column of water in the tube frequently defcended to 24 inches, and feldom rofe to 26: whence we may eftimate the mean height, which before was 26 inches, only 24¾.

Now this difference occafions a diminution of velocity in the efflux of the air, and confequently fhews the faultinefs of this method under the circumftances here mentioned.

IV. *Experiments on the Effect of Hatches placed near the Orifice of the Machine.*

Advantage of regulating the influx of water.

The hatch *a* Fig. 1. was replaced in the grooves adapted to the channel. We altered its height from the bottom of the channel, obferving the movements of the anemometer, in order to find the pofition moft favourable for the effect of the machine.

The mean height of the column of water in the tube never exceeded 28 inches, the elevation of the lower part of the hatch above the bottom of the channel, being then five inches one line; and it is remarkable, that the difference of a fingle line in this elevation lowered the water in the tube confiderably.

V. *Experiments on the Croffes when the Hatch is ufed.*

When thus regulated.

The hatch being placed as has juft been faid, we fitted the crofs again at the fuperior aperture of the funnel, when the water in the tube of the anemometer funk. We then varied the height of the hatch above the bottom of the channel, obferving the progrefs of the inftrument, to determine the moft advantageous pofition for it under the prefent circumftances.

The crofs produced more air,

The elevation of five inches eight lines was now found the moft favourable to the effect. With this the water ofcillated in the tube between 28 and 30 inches, moft frequently reaching the latter height, which we could never bring it to exceed, whatever changes we made in the arrangement of the parts that compofe the machine.

but expended more water.

If we compare the fituation of the hatch before the addition of the crofs, with that which is moft fuitable in the cafe before us, we find an increafe of feven lines in the height

2 from

from the bottom of the channel: now this addition to the height considerably increases the quantity of water expended by the machine.

Conclusions from these Experiments.

General conclusions. The engine by 173 cubic feet falling through 21 feet, drove out 441 cubic feet of air in a minute, under a pressure of 26 inches of water on nearly two inches of mercury; which is not quite one pound per square inch.

(A.) Under the circumstances related in the first set of experiments.

1. The expenditure of water for the blowing machine with which they were made was 173 cubic feet in a minute.

2. The air emitted from the aperture of the nozzle, being two inches in diameter, when the anemometer was at 26 inches, was 441 cubic feet in a minute.

(B.) Of the four air-holes in the machine, three only contribute to the effect produced.

(C.) The hatch placed near the orifice of the machine increased its effect, when the lower part of it was raised five inches one line above the bottom of the channel to which it was fitted.

(D.) A cross placed at the upper orifice of the machine diminishes its effect when the hatch is taken away: on the contrary they increase it, if the hatch be so placed, as to be five inches eight lines above the bottom of the channel, an elevation greater than that mentioned in the preceding paragraph (C.) and which increases the expenditure of water.

From these results it may be inferred, that the cross should not be used in several cases, where the quantity of water with which the machine is supplied, is confined within certain limits.

ANNOTATION. W. N.

The water blowing engine farther explained.

The blowing engine described in the preceding memoir acts upon the principle of the lateral adhesion of fluids, upon which Venturi has so ably written, in a treatise given entire in our Quarto Series of this Journal, and separately published afterwards by Taylor in Holborn. The shower of water in its descent through the vertical pipe carries down a mass of air along with it, in the same manner as a shower of rain on the flat surface of the sea produces that temporary blast of wind,

 which

which seamen term a squall, and is sufficiently violent to carry away the mass of a ship, if the sails be not reduced in time.

It is evident that this engine possesses the desireable qualities of cheapness and simplicity; and Lewis who has written somewhat fully upon it, in his Philosophical Commerce of Arts, from experiments of his own, asserts, that it requires much less water for working it than any other kind of bellows in use. I have no doubt but that many occasions must offer in which it would be beneficial; but whether its expence of water be comparatively small, and its power in any case equal to the supply of our smelting furnaces, may be deserving of more enquiry.

Numerical statement by Mr. Roebuck, of the effect of a steam engine in affording a blast of air,

In the excellent paper of Mr. Roebuck on the Devon iron works, inserted in the fifth Volume of the Edinburgh Transactions, and also in the Quarto Series of this Journal, there are some numerical facts respecting the blast afforded to iron furnaces by iron cylinder bellows worked by the steam engine; and as they agree very well with others given in my Chemical Dictionary, under the article *Trompe*, I will state them in this place. Mr. Roebuck affirms, that one iron furnace was excited by a blowing cylinder, which gave 155 cubic feet of air 16 times per minute, which numbers give a product of 2480 cubic feet. This is $5\frac{1}{2}$ times the quantity emitted by the blowing engine in the text. The steam engine was estimated to act by a pressure of 13032 lbs. answering to $2\frac{1}{4}$ lbs. on the square inch of the air piston, and this multiplied by four feet eight inches, the length of stroke, and by 16, the number of strokes, gives 975296 for the weight multiplied by its fall in feet.

compared with the water blowing engine.

Now the machine in the text was worked by 173 cubic feet, or 10812 lbs. of water falling through 21 feet, which gives a product of 227052, or more than one fourth of the first mover of the steam engine blast, instead of one fifth and a half. The blowing engine therefore consumed more water by one fourth than would have been required to produce its effect, according to what was done by the steam engine But the steam engine drove out its air under a reaction of between five and six inches of mercury in the gage; a velocity which being more than Mr. Roebuck found necessary, was a disadvantageous waste of power. The velocity of the water blowing engine produced by its pressure of two inches, is most probably too small; and if so, the multiplication of

The steam engine has more power.

these

these engines would not be adviseable, even if Lewis had been in the right in supposing them to save water. These rough computations, or rather estimates, are sufficiently near for data so loose as those upon which we have operated; and they appear to shew that the principal, and perhaps the only recommendation of the water engines is, that many of them may be made and applied at a small charge, in situations where water with a proper fall is plentifully to be had.

XII.

A Method of rendering the long and short Vibrations of a Balance, governed by a spiral Spring, precisely equal in Duration. By Mr. CHARLES YOUNG. *In a Letter from the Inventor.*

To Mr. NICHOLSON.

SIR

No explanation has been given why the long and short vibrations of a balance are different.

I HAVE lately tried many experiments upon springs, with a view to obtain some knowledge of the causes which govern an effect that is very troublesome to all makers of chronometers; namely, that the vibrations of the balance through short arcs, consisting of perhaps ninety degrees, are in some instruments performed in longer, and in others in shorter times than those long arcs, such as of four hundred. It is certain that no satisfactory reasons have been given, either in England or in France, to show how this irregularity is produced.

A balance suspended by a strait wire had its long and short vibrations equal.

I made a piece of brass to serve as a large watch balance, and suspended it by a bit of spring wire, on which it could vibrate as an axis, then having turned it four or five times, I left it to regain its natural position*. It performed all its

* This method of suspension has been used for philosophical purposes, by Mr. Mitchell, (see Priestley's Optics,) by Mr, Cavendish, (see Philos. Trans. and also this Journal quarto II. 446.) by Mr. Coulomb, in his numerous experiments on Electricity and Magnetism; and by Mr. Berthoud, in his Time Piece, No. 24. See his Treatise de la Measure du Temps, p. 50. It does not appear that this spring has yet been used by itself in time pieces. N.

oscillations

oscillations precisely in equal times, whatever was their extent, whether they consisted of thirty degrees, or of three thousand. It therefore returned to the place at which it was at rest with velocity exactly proportioned to the forces employed to remove it. From this experiment I concluded, that the balance spring of a watch is not in a situation to exert this natural quality, but that the distortion into which it is thrown, is such as destroy this valuable property of isochronism.

The principal circumstance by which the spiral balance spring appears to me to be cramped, and prevented from operating by its natural action throughout, is, that the outer extremity is fixed by the stud, so that it cannot expand and contract in its coils every where alike, as it ought to do. To remedy this, I attached the stud to a straight spring, lying in the direction of the tangent of the spiral, continued from that extremity. This spring by its easy action allows the spiral to approach the centre, and retire from it with great regularity; and, what is most material, it can with certainty be reduced to such a strength, that the long and short vibrations of the balance will prove perfectly equal when this adjustment is made. For upon the strength of this short spring depends the freedom with which the axis of the balance is enveloped by that spring which regulates its motion. The spring stud affords a good banking; for the banking pin on the balance may be easily placed so as to strike upon the end of the stud in the case of extreme vibration.

The spiral spring is cramped by the stud.

but when its outer end is made free by a straight spring its vibrations are all isochronal.

I am, Sir,

Your's most respectfully,

CHARLES YOUNG.

Wood Street, Aug. 23, 1805.

SCIENTIFIC

SCIENTIFIC NEWS.

Composition of Muriatic Acid.

Letter on the composition of muriatic acid.

IN the third number of the Edinburgh Medical and Surgical Journal, published July 1 last, is the translation of a letter forwarded to the editors of that work by the celebrated Fabbroni. It bears date from Pisa, May 9, 1805, and is written by Dr. Francisco Pacchioni, professor of natural philosopy in the university of that city, to Sig. Lorenzo Pignotti.

Water decomposed by galvanism shewed

After some prefatory observations, the writer announces that he has succeeded by galvanism in obtaining satisfactory evidence of the nature of the constituent principles of muriatic acid. He expresses his confidence that the simplicity of his apparatus and means have secured him against illusion; but for want of time he forbears to relate the whole series of his experiments. His results are,

—that muriatic acid is an oxide of hidrogen.

1. Muriatic acid is an oxide of hidrogen. 2. In the oxigenated muriatic acid and therefore, *à fortiori*, in muriatic acid there is a much less proportion of oxigen than in water. 3. Hidrogen may have very many and different degrees of oxidation.

Some account of the experiment.

The author informs us that having, by accurate experiments, ascertained the true theory of galvanism, he readily discovered a very simple and exact apparatus, in which he could distinctly perceive the changes which happen to water, which, from the continued action of the galvanic pile, is constantly losing its oxigen at the surface of a wire of very pure gold immersed in it.

Water deprived of oxigen first became acid;

With this apparatus, which I conjecture must have been the same as that of Davy, in which the oxigen and hydrogen were given off in separate vessels of water, he observed that pure oxigen was emitted from the gold wire, that the water became acid, and when by proceeding in the operation until the residual fluid occupied about half the capacity of the receiver (that is, I presume, when half the fluid in one of the vessels had disappeared) the remainder was found to be of an orange colour, more deep the less quantity of fluid. It resembled a solution of gold. From the lower orifice of the vessel, which was closed with a piece of taffetas and then with double bladder, a smell was emitted of oxigenated muriatic acid. The gold wire appeared corroded. The bit of taffetas which

—and then appeared to have dissolved part of the gold wire.

which had been in contact with the coloured fluid had undergone an action which rendered it easily to be torn. Round the edges of the vessel on the bladder there was a deep purple ring and within that a circular space rendered colourless or white. A drop of the fluid itself tinged the skin of the hand after some hours, of a beautiful rose colour.

Qualities of the fluid.

It contained muriatic acid.

The same liquid, possessing constantly the same qualities, was obtained in various repetitions of the experiment. It was shewn to contain a volatile acid by the white vapours which were formed by ammonia placed near it. It threw down a curdy precipitate from nitrate of silver, which the author concludes to have been the muriate of that metal; and from the whole of the facts he deduces the results first enumerated at the beginnig of this abstract, respecting the composition of muriatic acid from water by depriving it of part of its oxigen. He promises to treat of the other oxides of hydrogen in a memoir shortly to appear.

Hence the origin of the salt of the sea.

The origin and nature of the muriatic acid being thus, as the author observes, determined, there is no longer any mystery in its formation, nor in that of the muriatic salts in the vast extent of the ocean.

Acid and alkali observed by Cruickshank in 1801 to be formed by galvanism,

—and common salt by Peele in 1805,

Or muriate of soda.

The editors of the respectable Journal, from which I have made this extract take notice of the early discovery of Cruickshank (published by him in our quarto series for 1801) that infusion of litmus was reddened by one end of the pile and infusion of Brazil wood rendered purple by the other, which he ascribed to the formation of nitrous acid and ammonia; and they also quote the discovery of muriatic acid being formed by the galvanic action by Mr. Peele of Cambridge, which was announced in Mr. Tilloch's Philosophical Magazine a few days before Professor Pacchioni's letter was published at Pisa. Mr. Peele's letter bears date April 23, 1805. He took a pint of distilled water and decomposed half of it by means of galvanism; the other half, being then evaporated, left a small quantity of muriate of soda or common salt. Great attention had been paid to the purity of the water; and upon a careful repetition the same result was again had. In a postscript he mentions that a friend of his had tried the experiment and succeeded in the same manner.

Literary

Literary and Philosophical Society of Newcastle upon Tyne.

Twelfth report of the Newcastle Lit. and Phil. Society.

THE Literary and Philosophical Society of Newcastle-upon-Tyne have published their twelfth year's report. The spirited union of literature, science and practical research continues to form the character of their proceedings. Their library encreases no less in value than in magnitude, and they have liberally resolved "that the subscribers to the public library at North Shields (and to other similar institutions which "shall afford an equal accommodation to the members of the "Newcastle Society) shall be admitted to the rooms without "introduction on producing to the librarian a certificate of "their being members of such institutions." I will not suppose that any of my readers will consider this information as merely local. The advantages of provincial societies of estimable and well informed men is of high national importance, and it cannot but be of general interest that such enlightened proceedings as are adopted in one part of the kingdom should be known and imitated in every other quarter, where similar circumstances may render them desirable.

Blasting rocks in Mr. Jessop's method,

I have much pleasure in adding the testimony of Northumberland in favour of the improvement in blasting rocks, which Mr. Jessop communicated last December, through the channel of our Journal.

——tried with success in Northumberland.

At * the meeting in April, 1804, Mr. Fogget of Sheriff-Hill reported, that the new mode of blasting with sand, described in the Philos. Journal had been tried by him, and that, contrary to his expectation, it had answered every purpose of the old mode, with a considerable saving of powder, and of more than one-third of the labour, and with an entire freedom from risk.

At the meeting in May, Mr. Fogget presented a section of two holes drilled and prepared for blasting according to the new method: One perpendicular, in which the charge of powder being introduced and the communication-straw placed, the remainder of the hole is filled up with fine dry sand: the other a horizontal or ascending hole; in which the powder and sand, being made up into a cartridge, is in the act of being thrust up to the farthest extremity of the hole, by a blunt-pointed pricker put in by the side of the communication straw.

* Report, page 5.

At

At the meeting in June an account was communicated by Mr. Thornhill of an accident having happened in Gateshead-Park colliery, by which one man lost his life and another had been severely wounded, in consequence of the powder having taken fire in the common mode of stemming, or ramming down the charge with fragments of stone. A case was also cited by Mr. Horn of a person who had lately been brought from Alston Moor with his skull fractured by a similar explosion. Danger of the old method of ramming down the charge.

New Process for Steeping Hemp.

THE new process of M. Bralle for steeping hemp, which has the advantage of saving time, capital, and the health of numerous individuals, and of which an account was given at page 86 of our last volume, has been repeated in one of the provinces of France, to the entire satisfaction of the inhabitants, who might be supposed the least inclined to deviate from their accustomed habits. The staple was found to be excellent, and of a superior strength and quality when spun into thread, and also after it had passed the loom in the form of cloth. Steeping of hemp.

Medical Theatre, St. Bartholomew's Hospital.

THE following courses of Lectures will be delivered at this theatre during the ensuing winter.

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and physiology, by Mr. Abernethy.

On the theory and practice of surgery, by Mr. Abernethy.

On comparative anatomy and physiology, by Mr. Macartney.

On chemistry, by Dr. Edwards.

On the materia medica, by Dr. Powell.

Anatomical demonstrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Tuesday, October 1st, at two o'clock, and the other lectures on the succeeding days of the same week.

Further particulars may be learned by applying to Mr. Nicholson, at the apothecary's shop, St. Bartholomew's hospital.

AN

Medical Inſtitution.

AN inſtitution has been lately eſtabliſhed in London for the purpoſe of promoting a liberal and uſeful intercourſe among the different branches of the medical profeſſion, and of affording a centre for the reception of communications, and for the formation of a ſelect and extenſive profeſſional library. It is called the *Medical and Chirurgical Society of London,* and it comprizes a conſiderable number of profeſſional men of the firſt character. The meetings (which will commence in October) will be held at the Society's apartments, Verulam-buildings, Gray's-Inn, where any communications, or donations of books are requeſted to be ſent, directed to the ſecretaries.

The following is a liſt of the officers and council for the preſent year.

PRESIDENT, WM. SAUNDERS, M.D. F.R.S.
John Abernethy, Eſq. F.R.S. *Vice-Pres.*
Charles Rochemont Aikin, Eſq. Sec.
Wm. Babington, M.D. F.R.S. *Vice-Pres.*
Matthew Baillie, M.D. F.R.S.
Thos. Bateman, M.D. F.L.S.
Gilbert Blane, M.D. F.R.S.
Sir Wm. Blizard, F.R S. *Vice-Pres.*
John Cooke, M.D. F.A.S. *Vice-Pres.*
Aſtley Cooper, Eſq. F.R.S. *Treas.*
James Curry, M.D. F.A.S.
Sir Walter Farquhar, Bart. M.D.
Thompſon Forſter, Eſq.
Algernon Frampton, M.D.
John Heaviſide, Eſq. F.R.S.
Alex. Marcet, M.D. *For. Sec.*
David Pitcairne, M.D. F.R.S.
Hen. Revell Reynolds, M.D. F.R.S.
H. Leigh Thomas, Eſq.
James Wilſon, Eſq. F.R.S.
John Yelloly, M.D. *Sec.*

Properties

Properties of blued Steel not generally known.

Method of making ſprings.

IN making ſprings of ſteel the metal is drawn or hammered out and faſhioned to the deſired figure. It is then hardened by ignition to a low red heat and plunging it in water, which renders it quite brittle. And laſtly, it is tempered either by blazing or blueing. The operation of blazing conſiſts in ſmearing the article with oil or fat, and then heating it till thick vapours are emitted and burn off with a blaze. I ſuppoſe this temperature to be nearly the ſame as that of boiling mercury, which is generally reckoned to be at the 600° of Fahrenheit, though, for reaſons I ſhall in future mention, I think this point requires to be examined. The operation of blueing conſiſts in firſt brightening the ſurface of the ſteel, and then expoſing it to the regulated heat of a plate of metal or a charcoal fire, or the flame of a lamp until the ſurface acquires a blue colour by oxidation. The remarkable facts which I have here to preſent to the notice of philoſophers are that Mr. Stodart aſſures me that he has found the ſpring or elaſticity of the ſteel to be greatly impaired by taking off the blue with ſand paper or otherwiſe; and, what is ſtill more ſtriking, that it may be reſtored again by the blueing proceſs without any previous hardening or other additional treatment.

Hardening, blazing and blueing.

A blue ſpring injured by brightening and reſtored by blueing again.

Mr. Hardy, who is meritoriouſly known as a ſkilful artiſt,* aſſured me ſome time ago that the ſaw-makers firſt harden their plates in the uſual manner, in which ſtate they are more or leſs contorted or warped, and are brittle;—that they then blaze them; which proceſs deprives them of all ſpringineſs, ſo that they may be bended and hammered quite flat, which is a delicate part of the art of ſaw making;—and that they blue them on an hot iron which renders them ſtiff and ſpringy without altering the flatneſs of their ſurface. Mr. H. finds that ſoft unhardened ſteel may be rendered more elaſtic by blueing, and that hard ſteel is more expanſible by heat than ſoft.

Saw makers harden their ſteel; then ſoften it by blazing; and render it elaſtic by blueing.

Soft ſteel blued. Hard ſteel expands more by heat.

It is very difficult to reaſon or even to conjecture upon theſe facts. They certainly deſerve to be verified by a direct proceſs of examination, which I intend to make, and ſhall ſtate the reſults in our next number. N.

* See his banking for time pieces in our XI. Vol. page 114.

Preſervation of Succulent Plants.

Whether the fact be generally in poſſeſſion of the collectors of plants I know not, but it will certainly be inſtructive to many readers to be informed that green ſucculent plants are much better preſerved after a momentary immerſion in boiling water than otherwiſe. This treatment, which I am told is adopted for the economical preſervation of cabbage and other plants which are dried for keeping, deſtroys the vegetable life at once, and ſeems to prevent an after proceſs of decay or mortification, by which the plant would have been more conſiderably changed, if it had not been ſo ſuddenly killed.

Corrections to the 11th Volume.

P. 159, l. 2, *area* r. *era*.—l. 21, *Agy* r. *Aqy*.—l. 36, $q \times p - 2n$ r. $q + p - 2n$.—l. 12, *for* r. *of*.—l. 28, put a comma before *that*.—p. 236, l. 20 from the bottom, after the words *preſerved as* add *as perfect as poſſible, but the preſs recommended by Dr. Withering does not appear calculated*, &c.—

Dr. Boſtock's eſſay upon animal fluids which was communicated by the author and inſerted in our laſt number, appeared alſo in the third number of the Edinburgh Medical and Surgical Journal. This by a caſual omiſſion of the friend who forwarded me manuſcript was not intimated to me until ſeveral days after the paper came to hand.

Engine for raising Water by Mr. Wm. Close.

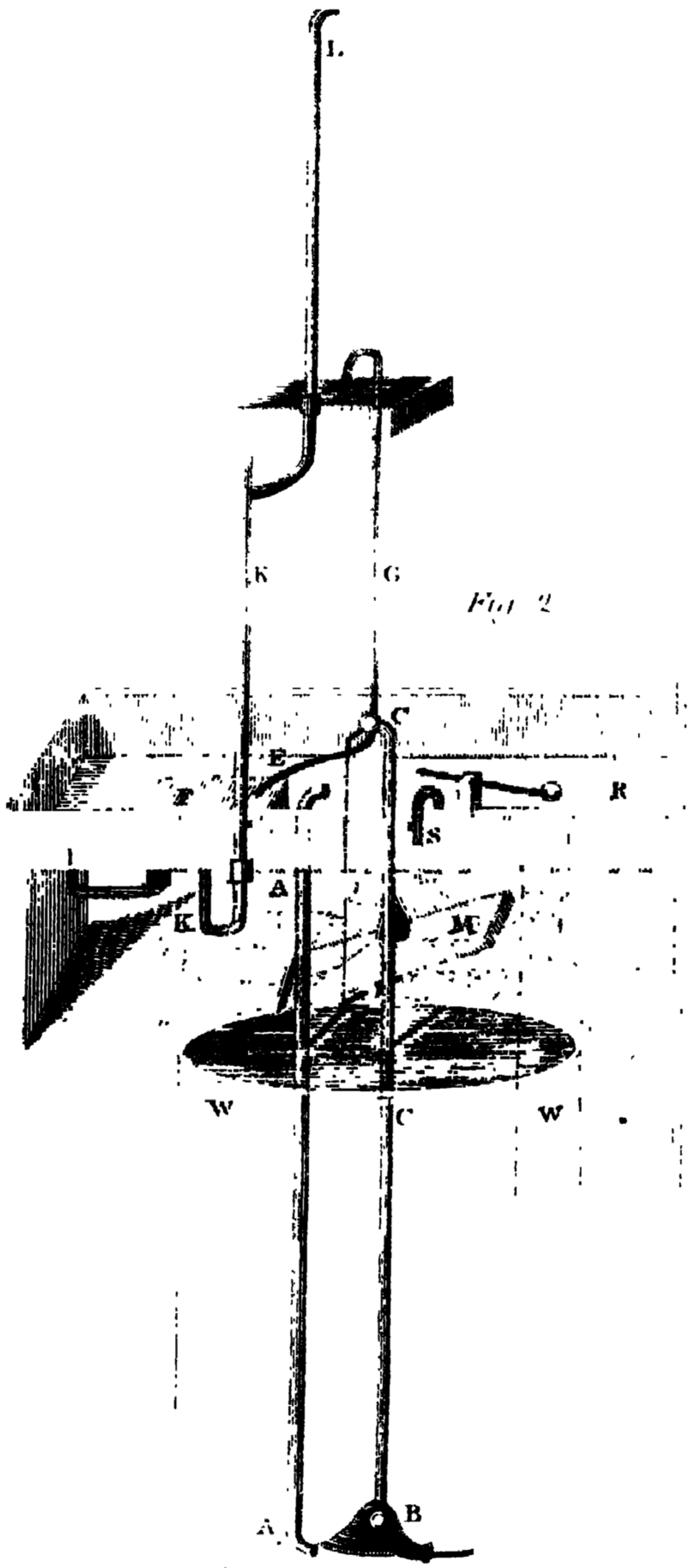


Fig. 2

Furnace for bending Wood.

Fig. 1.

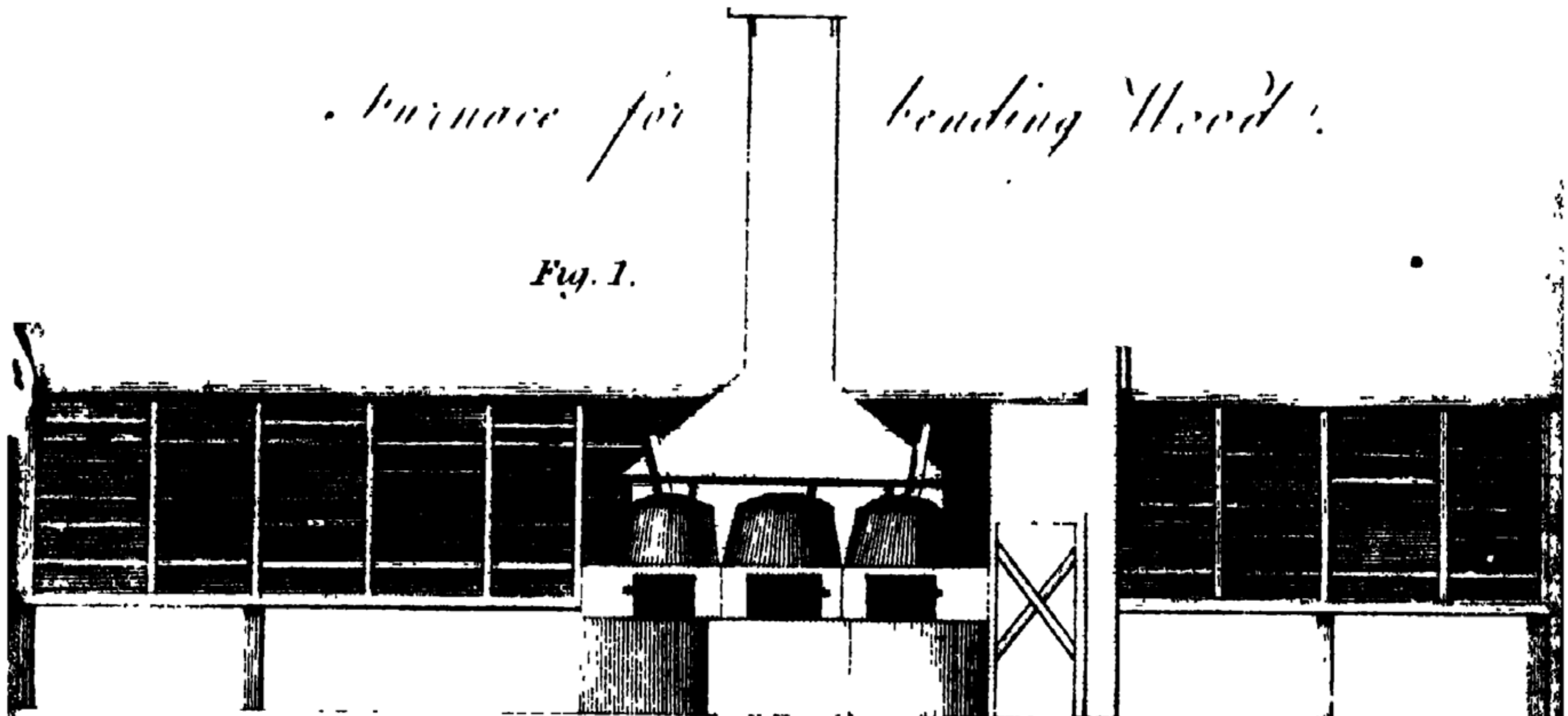


Fig. 2

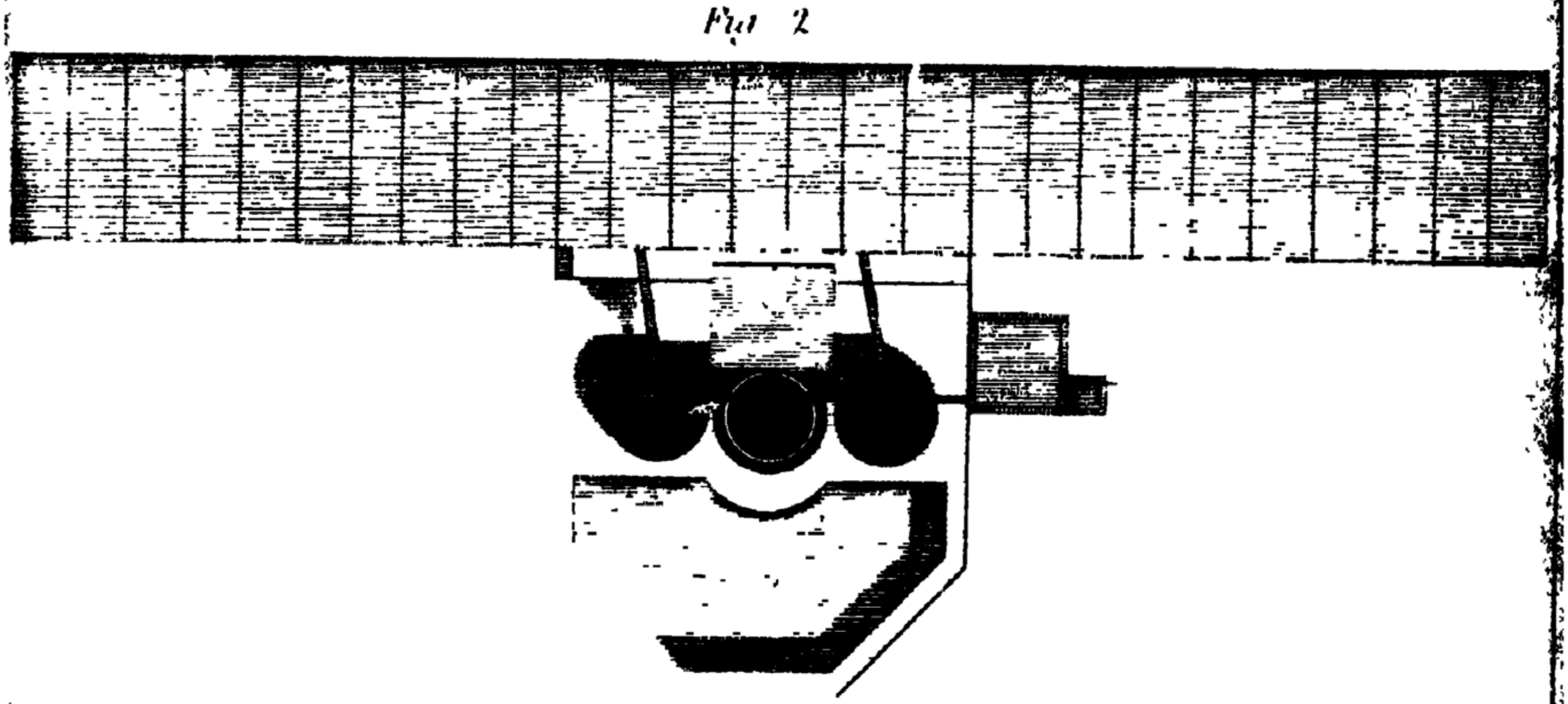
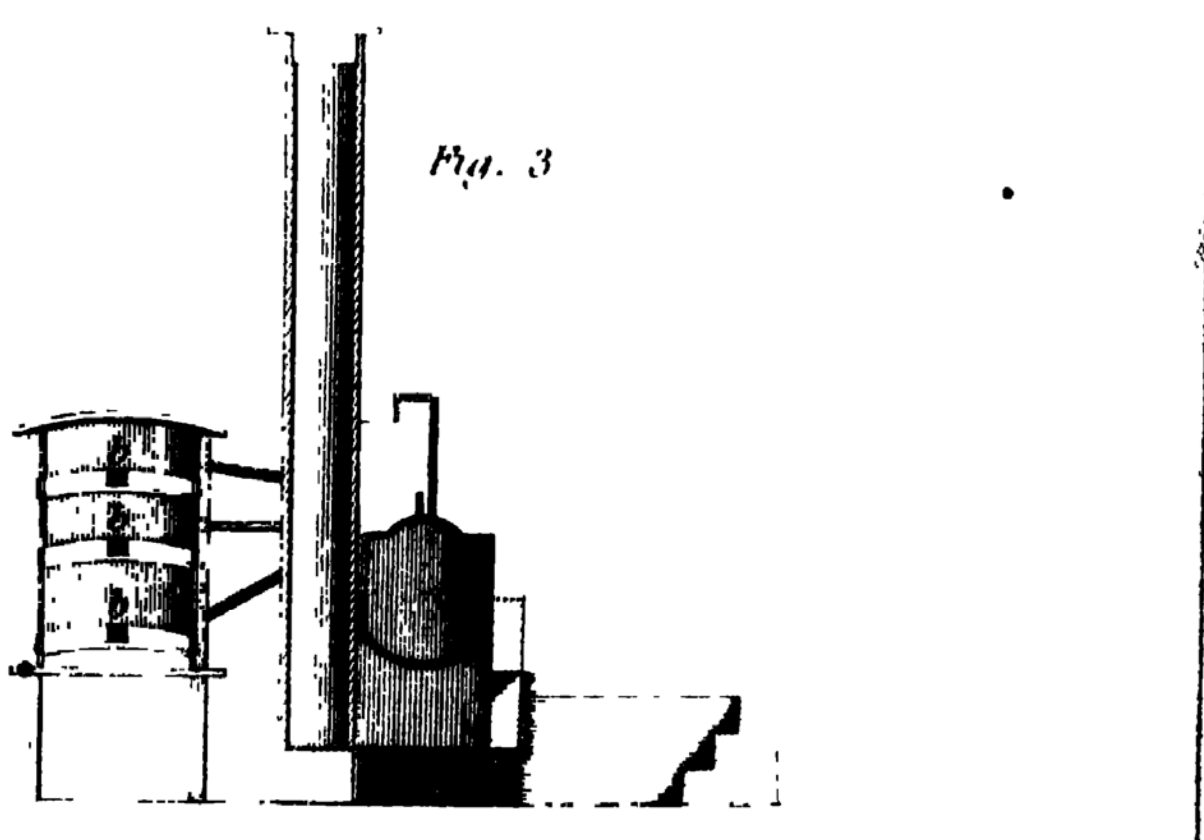


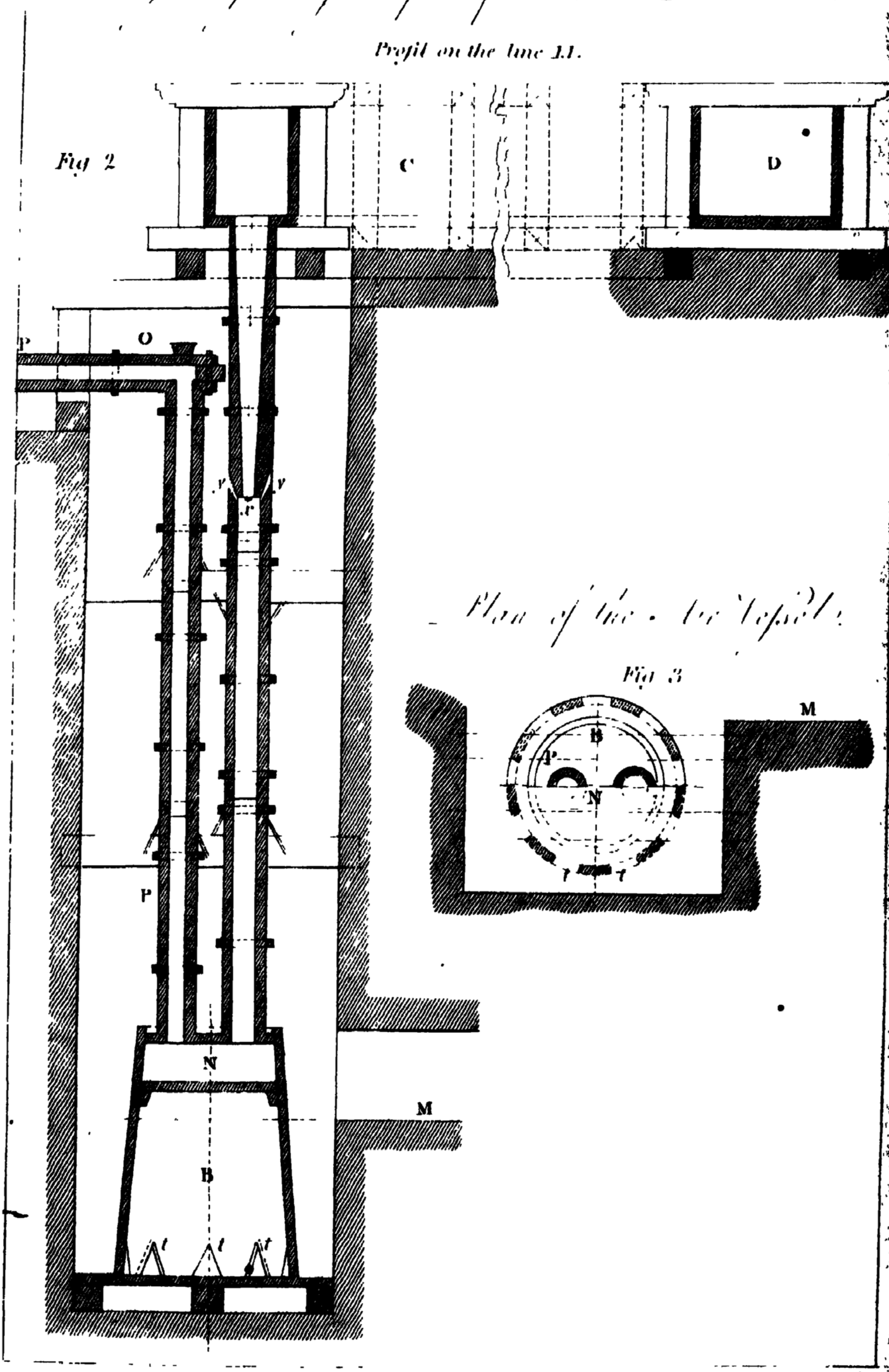
Fig. 3



Mutlow Sc Russell Ct

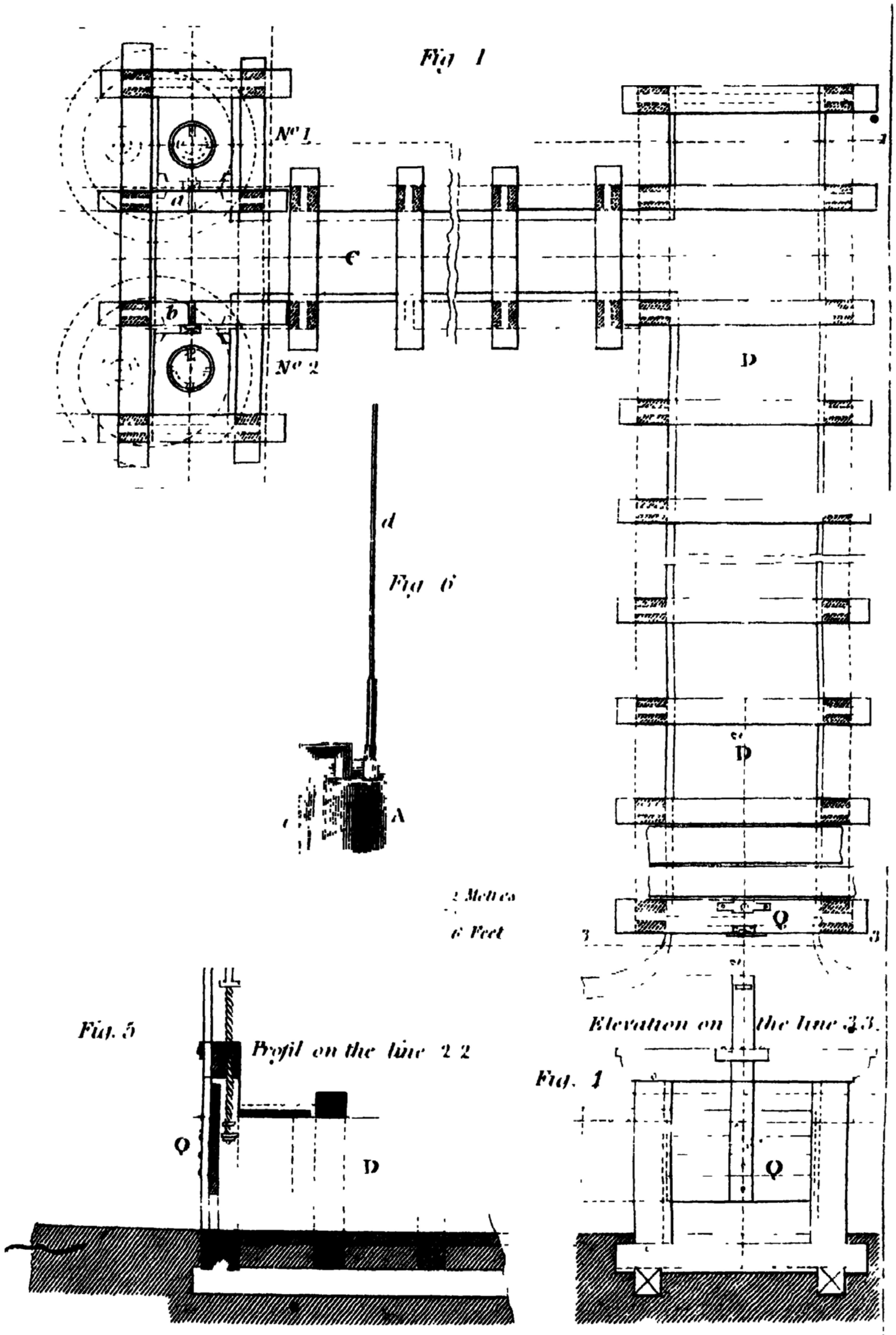
Blowing Engine by the fall of Water at Poullaouen

Profil on the line A.A.



Mutlow sc. Russ. Rd. Grd.

— Plan of the Water Ways)



A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

OCTOBER, 1805.

ARTICLE I.

Experimental Investigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c. Received from the Author.*

To Mr. NICHOLSON.

DEAR SIR, *Munich, August* 29, 1805.

HAVING learned by a letter which I received this day from England, that you have published in your Journal of Natural Philosophy the memoir I sent you on the temperature at which the density of water is a maximum, I take the liberty to send you herewith inclosed three memoirs on heat, which are destined to appear in the next volume of the publications of the first class of the National Institute.—Three other memoirs of mine will appear in that volume, but as they contain little that would be new to you, I do not send them to you.

I continue my researches on heat, and have lately made several new and very interesting experiments, the results of which it is my intention to communicate to you, as soon as I shall have compleated the particular course of experiments in which I am now engaged.

I am, Dear Sir, with much esteem,
Your most obedient servant,
RUMFORD.

SECT. I. *Short Account of a new Experiment on Heat.*

I have lately made a new experiment, the refult of which appears to me fufficiently interefting to deferve the attention of the clafs.

Qu. Whether the heating and cooling of polifhed and of blackened bodies follow the fame law in fmall clofed fpaces as in fpaces more enlarged?

Having found by experiments often repeated that metallic bodies expofed in the free air of a large apartment are much more fpeedily heated and cooled when their furfaces have been blackened (over the flame of a candle for example) than when they are clean and polifhed; I was curious to know whether the fame phenomena would take place when, inftead of expofing thefe bodies in the open air, they fhould be placed in clofe metallic veffels furrounded by a certain thicknefs of included air, and thefe veffels fhould be then plunged in a large mafs of hot or cold water. In order to clear up this important point, I made the following experiment:

A cylindrical veffel of thin brafs was fupported in the middle of a larger veffel, fo as to leave a thin interval of air between them.

A cylindrical veffel of brafs, three inches in diameter and four inches long was enclofed in another larger cylindrical veffel, in the centre of which it was fufpended by its neck, fo as to touch it in no other part, leaving on all fides an interval of one inch between the veffels.

The external veffel as well as the fmaller one included within it is made of thin fheets of brafs; its diameter is five inches and its height fix. It is one inch and a half in diameter and fix inches high. Its neck is one inch and a quarter in diameter and two inches and a half long.

The interior veffel is fufpended in the centre of the external one by a ftopper of cork. This ftopper is adjufted to the neck of the external veffel, and there is a cylindrical hole of three quarters of an inch diameter through the cork, and having the fame axis, which perforation receives the neck of the interior veffel and retains it in its place.

The interior veffel was introduced and fixed in its place before the bottom of the exterior veffel was foldered in.

The larger veffel was fupported on a foot.

At the centre of the bottom of the great veffel is a fmall metallic tube of three quarters of an inch diameter and one inch and a half long, by means of which this inftrument is attached to a folid heavy foot of metal which fupports it in a vertical pofition when the whole inftrument is fubmerged in a veffel of water.

This inftrument, which greatly refembles that defcribed in my 7th effay on the propagation of heat in fluids, which I have called

called the Paffage Thermometer*, may be ufed to make a number of interefting experiments on the cooling of bodies through different fluids. In the prefent experiment I employed it in the following manner:

The inner veffel was filled with hot water, and a thermometer placed in its neck.

The interior veffel was entirely filled with hot water to the height of half an inch in its neck, and a good thermometer, having its cylindric bulb four inches long, was inferted therein. The inftrument was then plunged in a mixture of pounded ice and water, and the time was noted by means of the thermometer, during which the hot water in the fmall veffel became cold.

The whole inftrument was then plunged in ice and water.

I was careful to plunge the inftrument in this frigorific mixture, fo that the large veffel was completely fubmerged except the upper extremity of its neck; and I added from time to time a fufficient quantity of pounded ice, to keep the frigorific mixture conftantly and throughout at the temperature of melting ice.

The following were the refults afforded by two fimilar inftruments employed at the fame time:

Refults with two inftruments one of which, B, had the interior furface of the larger veffel and the exterior of the fmaller blacked; and the other inftrument, A, had the like furfaces polifhed.

Thefe two inftruments, which I fhall diftinguifh refpectively by the letters A and B, are of the fame form and dimenfions; there is no difference between them but in the ftate of their furfaces. In the inftrument A, the exterior furface of the fmall veffel and the interior furface of the great veffel which incloses it, are bright and polifhed; but in the inftrument B, the exterior furface of the fmall veffel and the interior furface of the large veffel are black, having been blackened over the flame of a candle before the bottom of the great veffel was foldered in its place.

The interior veffels contained boiling water.

Having filled the interior veffel of each of thefe inftruments with boiling water till the water rofe to the height of half an inch in the neck, I placed a thermometer in each, and then plunging both inftruments at the fame time into a tub filled with cold water mixed with pounded ice, I obferved the courfe of their refrigeration during feveral hours.

The refrigerating veffel contained ice and water.

Each of the inftruments was completely fubmerged in the frigorific mixture, excepting about one inch of the fuperior extremity of the neck of the exterior veffel, and I was careful

* See our Journal, Vol. IX.

to add new quantities of pounded ice from time to time, in order to keep the frigorific mixture conſtantly at the preciſe temperature of melting ice.

Caution to inſure equality of temperature.

As the ſpecific gravity of water at the temperature of three or four degrees of the thermometer of Reaumur, is greater than that of melting ice, the water which lies at the bottom of a veſſel containing a mixture of water and pounded ice, is uſually warmer than the fluid which occupies the upper part of the veſſel. To remedy this inconvenience my refrigeratory for the frigorific mixture was a tin veſſel ſupported on three feet of one inch in length, and I placed this firſt veſſel in a larger of wood, containing a certain quantity of ice ſurrounding the bottom and part of the ſides of the metallic veſſel.

Method of obſervation.

As in the firſt moments of the experiment the thermometers deſcended too quickly to be obſerved with preciſion, I waited till each of them had arrived at the 55th degree of Reaumur; after which I carefully obſerved the number of minutes and ſeconds employed in paſſing through each interval of five degrees of the lower part of the ſcale of the thermometer to the fifth degree above zero.

Table of the courſe of cooling.

The following table exhibits the depreſſion of the thermometers during eight hours employed in the experiment.

Degrees of the Thermometer.		*Time employed in cooling,* By the Inſtrument A.			By the Inſtrument B.	
From 55	to 50	11m	6s	-	7m	50s
50	45	13	15	-	8	10
45	40	15	12	-	9	5
40	35	19	10	-	10	50
35	30	22	24	-	12	18
30	25	27	50	-	15	10
25	20	37	6	-	21	15
20	15	54	15	-	28	15
15	10	80	25	-	41	25
10	5	183	45	-	85	15
Time employed in cooling from 55° to 5°,		478	4	-	254	

It

It is evident from the results of this experiment, that the blackened body is constantly cooled in less time than the polished body; but it appears by the course of the thermometers, that the difference between the quickness of cooling of these two bodies varies, and that this difference was less considerable in proportion as the temperature of the bodies was more elevated in comparison to that of the medium in which they were exposed to cool.

The blackened body always cooled more quickly than the other.

In cooling from the 55th degree to the 50th above the temperature of the surrounding medium, the polished body employed 11ᵐ 6ˢ, and the blackened body employed 7ᵐ 50ˢ to pass through the same interval. But from the 10th to the 15th degree above the temperature of the medium, the polished body employed 183ᵐ 45ˢ, while the blackened body employed only 85ᵐ 15ˢ; but it is extremely probable that this difference between the proportion of the times employed in cooling the two bodies at different temperatures, is only apparent, and that it depends on the greater or less time required for the thermometers in the vessels to arrive at the mean temperatures of the masses of water which surround them.

The difference was greatest at the lowest temperatures; probably because the thermometers then shewed the mean temperature more correctly.

In order to compare the results of this experiment with those I made last year with metallic vessels polished and blackened, and left to cool in the undisturbed air of a large chamber, it is necessary to ascertain how much time the two bodies in question employed in cooling, from the 50th to the 10th degree of Fahrenheit above the temperature of the medium. Now I found by observation, that the polished vessel A employed 39ᵐ 30ˢ to pass over that interval of cooling, while the blackened vessel B employed only 22ᵐ. These times are in the proportion of 10000 to 5810. By one of my experiments made last year, I found that the times employed in passing through the same interval of cooling in the open air by a clean polished metallic vessel, and another of the same form and capacity, but blackened without, were as 10000 to 5654.

From these experiments it appears that the rate of cooling in the polished body, compared with the other, is nearly the same as was formerly determined with bodies in a large chamber.

Reflecting on the consequences which ought to result from the radiations of bodies, on the supposition that the temperatures of bodies are always changing by means of these radiations, I was led to the following conclusion: If the intensity of the action of the rays which proceed from a body, be universally as the squares of the distances of bodies inversely,

If the intensities of radiated heat be inversely as the squares of the distances, bodies will cool in the same time in an enclosure of the same tem-

perature, whether large or small.

which is extremely probable, a hot body exposed to cool in a close place, or surrounded on all sides by walls, ought to cool with the same celerity, or in the same time, whatever may be the magnitude of this enclosure, provided the temperature of the sides or walls be at a constant given temperature; and the results of the experiment here described, in which the hot body was enclosed in a vessel of a few inches diameter, compared with those of several experiments made last year, in which the heated bodies exposed to cool between the walls of a large chamber, appear to confirm this conclusion.

These facts confirm that truth;

and that the air has little effect.

As to the effect produced by the air in cooling a heated body exposed to cool in a close place filled with that fluid, I have reason to believe that it is much less considerable than has been supposed.

Former experiments proved that it receives only 1-27th part.

I have shewn by direct and conclusive experiments, that bodies cool and are heated, and that with considerable celerity, when placed in a space void of air *; and, by experiments made last year with the intention of clearing up this point, I found reasons to conclude, that when a hot body cools in tranquil air not agitated by winds, one twenty-seventh only of the heat lost by this body (or to speak more corectly, which it excites in surrounding bodies) is communicated to the air, all the rest being carried to a distance through the air, and communicated by radiation to the surrounding solid bodies.

The rest passes by radiation.

Sect. II. *Experiments on cooling Bodies.*

It is only by careful observation of the phenomena which accompany the heating and cooling of bodies, that we can hope to acquire exact notions of the nature of heat and its manner of acting.

Conducting power of bodies with regard to heat.

Many experiments have been made by different persons at different times, with a view to determine what has been called the conducting quality of different substances with regard to heat: I have myself made a considerable number; and it is from their results, often no less unexpected than interesting, that I have been gradually led to adopt the opinions on the nature of heat which I have presumed to submit to the judgment of this illustrious assembly. The flattering attention

* See my Memoir on Heat in the Philosophical Transactions for 1786; and in my eighth Essay.

with

with which the Clafs has honoured the three Memoirs I have lately prefented, encourages me to communicate the continuation of my refearches.

Glafs is allowed to be one of the worft conductors.

All philofophers are agreed in confidering glafs as one of the worft conductors of heat which exifts; and when it is propofed to confine the heat in a body of which the temperature has been raifed, or to hinder its diffipation as much as poffible, care is taken to furround the heated body with fubftances known to be bad conductors of heat.

Bodies are not, probably, cooled by conducting off.

The refults of many of my experiments having led me to fufpect that the cooling of bodies is not effected in the manner which is generally fuppofed, I made the following experiment with the intention of clearing up this interefting part of the fcience.

Experiment: A thick glafs bottle and a thin one of tin,

I procured two bottles nearly cylindrical, of the fame form and the fame dimenfions when meafured externally; one being of glafs and very thick, and the other of tin or tinned iron, which was very thin. Each of them is three inches ten lines in diameter very nearly, and five inches in height, and each has a neck one inch three lines in diameter, and one inch two lines in height. The glafs bottle weighs 13 ounces 1 gros and 18 grains poids de marc, and the other thin metallic veffel weighs only 5 ounces 1 gros and 65 grains.

were prepared, weighed,

Having very exactly weighed the bottle of tinned iron, I found its exterior furface to be 54,462 inches, which give 0,21142 of a line for the thicknefs of its fides, taking the fpecific gravity of the metal at 7,8404.

and meafured.

The mean thicknefs of the fides of the glafs bottle is more than fix times as great, as may be eafily deduced from a calculation founded on the weight of the bottle, the quantity of its furface, and the fpecific gravity of glafs.

They were filled with hot water, and left to cool in the air.

Having filled thefe two bottles with boiling water, I hung them up by flender ftrings in the midft of the tranquil air of a large chamber, at the height of five feet from the floor, and at the diftance of four feet afunder.

The temperature of the air of the chamber, which did not vary a quarter of a degree during the whole time of the experiment, was 9¾ degrees of Reaumur's fcale.

An excellent mercurial thermometer, with a cylindrical bulb, of four inches long and two lines and a half in diameter, fufpended in the axis of each of thefe bottles, indicated the

temperature

temperature of the contained water; and the time employed in its cooling for every five degrees of Fahrenheit's thermometer, was carefully observed during eight hours.

The glass being considered as a very bad conductor of heat, and the sides of the bottle being so thick, who would not have expected that the water in this bottle would have been more slowly cooled than that in the very thin bottle of tin.

The glass bottle cooled twice as quick as that of tin.

The contrary however was the event; the bottle of glass was cooled almost twice as quickly as that of tin.

While the water included in the bottle of tinned iron employed 56 minutes to pass through a certain interval of cooling, namely through ten degrees, between the 50th and 40th degree of the thermometer of Fahrenheit above the temperature of the air of the chamber, the water in the glass bottle employed only 30 minntes for the same change.

Inference.

It appears to me, that the result of this experiment throws great light on the mysterious operation of the communication of heat.

Heat is not likely to be a material substance.

If we admit the hypothesis that hot bodies are cooled, not by losing or acquiring some material substance, but by the action of colder surrounding bodies, communicated by undulations or radiations excited in an etherial fluid, the results of this experiment may be easily explained; but if this hypothesis be not adopted, I cannot explain them.

Bodies are not cooled by the surrounding air.

It might perhaps be suspected that the air attached by a certain attraction, but with unequal forces, to the surfaces of the two bottles, might have been the cause of this remarkable difference in the time of their cooling; but those who will take the trouble to reflect attentively on the results of the experiments I have described in a preceding memoir, which were made with a view to clear up this point, with a metallic vessel first naked, and afterwards with one, two, four and five coatings of varnish, will be persuaded that this cause is not sufficient to explain the facts.

All metallic vessels have the same disposition to cool.

By a course of experiments made at Munich last year, of which the details are given in a Memoir sent to the Royal Society of London *, I have found that a given quantity of hot water included in a metallic vessel of a given form and capacity, always cools with the same quickness in the air,

* See our Journal, Vol. IX. p. 194.

whatever

whatever may be the metal employed to construct the vessel; provided always that the external surface of the vessel be very clean, and the temperature of the air the same.

The surface only need be metallic.

In order that the cooling shall be effected in the same time, nothing more is required than that the external surface of the vessel be truly metallic, and not covered with oxide or other foreign bodies.

This arises from the opacity of metals,

On the enquiry, what quality all the metals might have in common, and possess in the same degree, to which this remarkable equality of their susceptibility of cooling might be attributed, I found it in their opacity.

by which the heat is reflected.

The rays which cannot penetrate the surface of a body, must necessarily be thrown back or reflected; and as the rays of light, which have much analogy with the invisible calorific or frigorific rays, easily penetrate glass, though they are reflected, at least for the greatest part, by metallic surfaces, I suspected beforehand the result of the experiment with the two bottles, one of glass and the other of tinned iron.

Usual comparison of heat to water in a sponge.

The state of a heated body, or a body which contains a certain quantity of caloric, has been compared to that of a sponge which contains a certain quantity of water. Supposing this comparison to be just, we might compare the loss of heat by the emission of the calorific rays, to the loss of water by evaporation. Let us try if this comparison can supply us with the means of throwing some light on the interesting subject of our researches.

The same amplified.

Instead of the sponge filled with water, let us substitute the earth, and suppose for a moment that the earth is every where equally heated, and its surface in all parts covered with a bed of the same kind of soil equally moist.

If it were true, a rough surface would emit more heat than a smooth one.

As a square league in a mountainous country contains more surface or more superficial acres than a square league situated in the plain, it is evident that more water would be evaporated from the whole surface of the earth in a given time if the earth were covered with mountains, than if its surface were an immense plain, and consequently, that more caloric ought to be projected from the surface of any solid body broken with asperités, than from the surface of another body of the same form and dimensions, which is smooth or well polished.

This

But the facts are contrary.

This reaſoning appears to me to be juſt, and if I am not deceived, the concluſions which may be drawn from the facts in queſtion, well confirmed by experiment, ought to be conſidered as demonſtrative. I have taken every poſſible care to eſtabliſh theſe facts; and the reſults of all my experiments have conſtantly ſhewn that more or leſs perfect poliſh, or the greater or leſs brightneſs of the ſurface of a metallic veſſel, does not ſenſibly influence the time of its cooling.

A poliſhed and unpoliſhed veſſel of braſs cooled in the ſame time.

I took two equal veſſels of braſs and poliſhed the external ſurface of one of them as highly as poſſible; and I deſtroyed the poliſh of the other by rubbing it in all directions with coarſe emery. When theſe two veſſels were filled with hot water, I did not find that the unpoliſhed veſſel employed more or leſs time in cooling than that which was poliſhed.

Caution.

I was careful to waſh the ſurface of the unpoliſhed veſſel effectually with water before the experiment; as I knew that if I did not take the precaution of removing all the dirt which might be lodged in the aſperités of its ſurface, the preſence of theſe ſmall foreign bodies would influence the reſult of the experiment in a ſenſible manner.

A rough ſurface may reflect as much light as a ſmoother.

We ought carefully to diſtinguiſh thoſe ſurfaces which appear unpoliſhed to our eyes, but which in fact are not ſo, from thoſe which reflect little or no light.

Metals not leſs reflective for loſing their poliſh.

It is more than probable that the ſurface of a metal is always poliſhed, and even always equally ſo in all the caſes wherein the metal is naked and clear and clean, notwithſtanding all the mechanical means which may be uſed to ſcratch its ſurface and break the glare of its luſtre.

If the radiation of heat deſcend on ſurrounding bodies, it will be of no conſequence whether the radiating body be poliſhed or not.

Let us return to the compariſon of the evaporation of water from the ſurface of the earth, with the emiſſion of caloric radiating from the ſurface of a heated body, and let us ſuppoſe for an inſtant, that the evaporation of the water from the ſurface of the earth does not depend on the heat of the earth itſelf, but that it is cauſed merely by the influences of ſurrounding bodies, as for example, by the rays of light received from the ſun. It is evident that, in this caſe, the evaporation could not be ſenſibly greater in a mountainous country than in the plain; and by an eaſy analogy we ſee, that if hot bodies be cooled, not in conſequence of the emiſſion of ſome material ſubſtance from their ſurfaces, but by the poſitive action of rays

ſent

ſent to them by colder ſurrounding bodies the more or leſs perfect poliſh of their ſurfaces, ought not ſenſibly to influence the rapidity of their cooling.

This is preciſely what all my experiments concur to prove. I have long ſought, and with that patience which the love of the ſciences inſpires, to reconcile the reſults of my experiments with the opinions generally received concerning the nature of heat and its mode of action, but without being able to ſucceed.

Experiments prove this poſition.

It is in the hands of two of the moſt illuſtrious bodies of learned men that ever exiſted that I have thought it incumbent on me to depoſit my labours, my diſcoveries, my doubts, and my conjectures.

I am earneſtly deſirous of engaging the philoſophers of all countries to turn their attention towards an object of enquiry too long neglected.

The ſcience of heat is not only of great curioſity, from the multitude of aſtoniſhing phenomena it offers to our contemplation, but it is likewiſe extremely intereſting from its intimate connection with all the uſeful arts, and generally with all the mechanical occupations of human life.

Importance of the ſcience of heat.

Without a knowledge of heat it is not poſſible either to excite it with economy, or to direct its different operations with facility and preciſion.

(The Remainder in our next.)

II.

On pure Nickel, diſcovered to be one of the noble Metals, and on its Preparation and Properties. By J. B. Richter.*

repeatedly cryſtallizing ſulphate of ammonia and nickel, the whole of the cobalt, an extremely ſmall quantity excepted, will be ſeparated; but after this there ſtill remains ſome copper mixed with the ſalt. I have already announced that this metal may be ſeparated from the nickel by ſubliming the latter with ſal ammoniac; but at that time I had never obtained pure nickel. With the compound ſalt of nickel and ammonia

Cobalt ſeparated from ſulphate of ammonia and nickel by repeated cryſtallization.

Copper, by ſubliming with ſal ammoniac.

* Tranſlated from the *Journal de Chimie* of Van Mons, vol. VI. p. 183, March, 1805; and abridged by Van Mons from the *Allgemeines Journal der Chemie*, 1804, vol. III. p. 244.

Arsenic and iron left.

a little arsenic still remains; and there may be iron likewise, when we have been a little too sparing in the addition of nitric acid to the sulphuric solution of cobalt containing calx of nickel.

The triple salt decomposed by carbonate of potash.

I endeavoured to separate these extraneous metals in the humid way, but not with complete success. I decomposed by means of carbonate of potash the triple ammoniacal salt of nickel, free from iron, and as much as possible from cobalt: the precipitate still was visibly of a greenish blue. Having edulcorated it, and heated it to redness, it changed its colour, as it lost its carbonic acid, to a blackish gray, which however inclined evidently to a green. The water of edulcoration, which had a greenish appearance, was evaporated to dryness, and the residuum, after being heated red hot, was washed again. A green powder remained, which did not lose its colour in the fire, and consisted in great part of arseniate of nickel.

The metal reduced.

I mixed each of the two residuums separately with a fifth part of charcoal, and exposed them to the heat of a potter's furnace for eighteen hours in a cupel with a little porcelain glaze. The metallic buttons obtained differed a little from each other. Each endured a few blows with a hammer without cracking; but that of the latter residuum was much more white and fragile than that of the former, the colour of which approached that of steel and was slightly reddish. They were both attacked with avidity by nitric acid, and they were attracted by the magnet, but the former only weakly.

The result not pure nickel.

As from several effects on porcelain it appeared to me probable, that pure nickel was a noble metal, I dissolved afresh in nitric acid the whole quantity reduced, which amounted to several ounces, and evaporated the solution to dryness. I then poured water on the saline mass, and a beautiful green solution was formed; but a greenish white residuum remained, in which I easily detected the presence of iron, nickel, and arsenic acid.

The solution precipitated and exposed to a strong heat.

The solution, which beside arsenic, contained a considerable portion of copper, was precipitated by carbonate of potash, and the residuum, the colour of which was still very lively, though not so green as that of carbonate of copper, was well washed and exposed to a white heat. This changed its apple green colour to a deep green inclining to gray and brown. With a stronger heat the mass assumed a grayer brown, and at the

the ſame time appeared to coagulate. There were likewiſe portions of reduced metal in it, that could not be miſtaken.—I could not, however, accompliſh its fuſion in a wind-furnace, ſurmounted with a cupellating furnace dome, and having a long chimney. In conſequence, I divided it into ſeveral portions, which I expoſed in crucibles to the ſtrongeſt heat of a potter's furnace, in which capſules of the moſt refractory clay are frequently ſoftened.

Very refractory.

In thoſe crucibles which were placed where the porcelain is longeſt taking, the matter had experienced no change but a coagulation. In the other crucibles it had entered into complete fuſion, yet not into a liquid fuſion, and the crucibles had partly experienced the ſame effect. Here and there in the melted maſs metallic globules were found, the largeſt of which were the ſize of a ſmall nut, and the leaſt that of a cherry-ſtone. Their brilliancy was a mean between that of ſilver and that of Engliſh tin. The ſcoriæ were greeniſh brown, mixed with an amethyſt colour, and in ſome places a deep blue entirely like fuſed oxide of cobalt. The brown colour aroſe from the oxide of copper, which was completely vitrified, and the blue from that of cobalt. The green, on the contrary, proceeded from arſeniate of nickel, which, as I have learned by experience, ſtrongly reſiſts fuſion, without the addition of ſome combuſtible ſubſtance.

Fuſed in the ſtrongeſt heat of a potter's furnace.

Scoriæ.

Arſeniate of nickel.

I attempted to hammer the metallic globules on an anvil, and to my great ſatiſfaction I found that they poſſeſſed conſiderable malleability. They were eagerly attracted by the magnet.

The metal malleable, and magnetic.

As it was impoſſible to ſeparate the ſcoriæ with the hammer from the little globules to which they adhered, I collected them together by trituration and decantation, and expoſed them to fuſion afreſh. It was again complete only in the places of the furnace moſt heated.

Re-fuſion.

Convinced from theſe reſults, that nickel is reducible in the fire without the addition of any combuſtible matter, I attempted to reduce ſome oxide of this metal, obtained by the decompoſition of the triple ammoniacal ſalt of nickel, which during an uninterrupted labour of eighteen months, I had procured in a very large quantity. On this occaſion the ſame phenomena occurred as in the preceding reductions.

Reduced without any addition.

I repeated the melting till the metal had undergone a complete fuſion, and was found collected together in a button at the

A button of an ounce and half procured.

the bottom of the crucible. In one crucible which had been *exposed to the strongest heat, I obtained a button that weighed an ounce and a half.*

Best reduced alone.

I was less successful in my fusion when I mixed the oxide of nickel with porcelain glaze, or when I simply covered it with this glaze; so that I was convinced the best process was to reduce the oxide of nickel directly.

After much time and patience, I succeeded in obtaining several ounces of nickel, which I must consider as absolutely pure, and I shall now proceed to describe the principal characters that I have perceived in it in this state.—To begin with the external characters.

External character of pure nickel. Colour. Unchangeable in the air. Malleable.

The colour of pure nickel is a mean between silver and tin. It undergoes no alteration either from the air of the atmosphere or from the water in it: in other words it is insusceptible of being oxided by the air.

It is perfectly malleable; as it may not only be forged into bars when red hot, but hammered on the anvil while cold into very thin plates. This character removes nickel from the class of semi-metals to that of perfect metals.

Specific gravity.

Its density or specific gravity is pretty considerable: from repeated experiments with my hidrometer cast nickel weighs 8.279, and forged nickel 8.666.

Ductile.

Its tenacity likewise appears considerable, to judge from its great ductility. A piece of cast nickel, weighing five drams allowed itself to be flattened, but not without cracking, into a plate of 13 square inches Rhynland measure, which gives less than $\frac{1}{100}$ of an inch for its thickness. It might probably be drawn into a wire of the same tenuity.

Refractory.

The resistance of nickel to fusion is very considerable, and equals, if it do not surpass, that of manganese. The reader may have observed, from my attempts to fuse it, how difficult it is to obtain any thing decisive on this head.

A noble metal.

At a temperature sufficiently high the pure, oxide of nickel is reducible without the addition of any combustible matter. Its great resistance to fusion is the only cause why this reduction presents so many difficulties. Very little oxidation too is perceptible on keeping this metal in a state of incandescence: it is merely tarnished a little more than platina, gold, or silver. Nickel therefore belongs not to the class of perfect metals merely, but to that of noble metals.

Magnetic.

The action of the magnet on nickel is not only very considerable

derable, and little inferior to that on iron; but nickel becomes itself magnetical, or acquires polarity, by the touch, and even in part by striking it with a hammer, or filing it, with the precautions suitable for producing this effect. I discovered the latter property by presenting to the magnet a slip of forged nickel; when, notwithstanding it was polished by the file, it adhered more feebly to the magnet than other slips less polished; but on my presenting its other extremity to the magnet, it adhered to it with great force. It likewise attracted by either side not only iron needles, but plates of nickel half an inch square, which it caused to move about on a smooth table.

Its magnetic property weakened by copper, destroyed by arsenic.

The property which nickel possesses of becoming magnetic is not destroyed, though weakened by its alloy with copper; but arsenic destroys it completely. I had frequent opportunities of making this observation in the course of my experiments. Some nickel, from which I had separated the iron * and the arsenic in the humid way, and which I had afterwards reduced with the addition of a combustible substance, was malleable, and attracted the magnet, though not so forcibly as pure nickel. The same metal, purified with less care, was less malleable, and proportionally less attractable by the magnet. Repeated exposure of the metal to the most powerful heat of a porcelain furnace did not in the least restore to it this property.—

Magnetic and malleable in proportion to its purity.

Copper must be separated by fire.

Some experiments, which I shall hereafter relate, have convinced me, that copper cannot be entirely separated from nickel in the humid way, and that the only means of separating them is to reduce the cupreous oxide of nickel by fire.

Action of the acids upon nickel.

The sulphuric and muriatic acids have little action upon nickel. The oxide of this metal by the air does not dissolve even in the latter of these acids without the assistance of a strong ebullition. The most appropriate solvents of nickel are the nitric and nitro-muriatic acids. I have already mentioned, that impure nickel, particularly the cupreous, is attacked by the nitric acid with heat and vivacity. The action of the same acid on pure nickel is a little different, and particularly on the

Best mode of freeing it from iron.

* The separation of the iron succeeds best by a rapid evaporation of the nitric solution of the ferruginous nickel, by which the iron is precipitated in the form of an insoluble oxide. At the same time a little arsenic is separated in union with the iron. It is preferable, however, to separate the arsenic first, which is effected by the help of a nitric solution of lead. The lead is afterward to be precipitated by a solution of sulphate of potash.

hammered

hammered metal. I have poured nitric acid on nickel both in buttons and laminated, expecting a very active solution; but it has proceeded slowly, and I have even been obliged to have recourse to the heat of a spirit lamp to accelerate it. The dissolution however having appeared to cease, I decanted the liquid and poured on the residuum a fresh quantity of acid of the same strength as the preceding, when on a sudden such a brisk action came on, accompanied with the evolution of heat *, that I could not remove the capsule to the fire-place quickly enough.

I shall now go on to consider some of the characters of pure nickel in the state of oxidation.

Characters of oxide of nickel.

The nitric solution of pure nickel has a beautiful grass-green colour. Carbonate of potash separates from it a pale apple green precipitate. This precipitate well washed and dried is very light. A thousand parts of metallic nickel reduced to this precipitate weigh 2,927 parts.

Precipitate very light.

If this precipitate be exposed to a white heat it becomes of a blackish gray, scarcely inclining to green, and weighing only 1,285. On continuing the fire, the mass approaches the metallic state more and more, and becomes magnetic. This is effected much more speedily if the oxide be moistened with a little oil.

Oil promotes its reduction.

Precipitate by ammonia.

On adding caustic ammonia in excess to a nitric solution of nickel, a precipitate is formed, resembling in colour ammoniure of copper, but not so deep. This colour sometimes changes in a couple of hours to an amethyst red, and to a violet, which colours are converted into an apple-green on the addition of an acid, and again to a blue and violet on the addition of ammonia. If however we add to the solution of nickel a solution of copper, so as to produce no perceptible change, the colour of the precipitate formed by ammonia ceases to assume a red tinge, and the red colour of the ammoniure of nickel disappears on the addition of a little ammoniure of copper; whence it follows, that every precipitate of nickel by ammonia, which retains its blue colour, has copper combined with it.

Its being a noble metal questioned.

* From this it is difficult to believe that nickel, under favourable circumstances, would not become oxided by the combined influence of air and fire. VAN MONS.

On

III.

On the Analysis of Soils, as connected with their Improvement. By HUMPHREY DAVY, Esq. F. R. S. Professor of Chemistry to the Board of Agriculture and to the Royal Institution.

I. Utility of Investigation relating to the Analysis of Soils.

THE methods of improving lands are immediately connected with the knowledge of the chemical nature of soils, and experiments on their composition appear capable of many useful applications.

The importance of this subject has been already felt by some very able cultivators of science; many useful facts and observations with regard to it have been furnished by Mr. Young; it has been examined by Lord Dundonald, in his treatise on the connexion of Chemistry with Agriculture, and by Mr. Kirwan in his excellent essay on Manures: but the enquiry is still far from being exhausted, and new methods of elucidating it are almost continually offered, in consequence of the rapid progress of chemical discovery. Analysis of soils; attended to by Mr. Young, Lord Dundonald, and Mr. Kirwan.

In the following pages I shall have the honour of laying before the Board, an account of those methods of analysing soils which appear most precise and simple, and most likely to to be useful to the practical farmer; they are founded partly upon the labours of the gentlemen, whose names have been just mentioned, and partly upon some later improvements.

II. Of the Substances found in Soils.

The substances which are found in soils, are certain mixtures or combinations of some of the primitive earths, animal and vegetable matter in a decomposing state, certain saline compounds, and the oxide of iron. These bodies always retain water, and exist in very different proportions in different lands; and the end of annlytical experiments is the detection of their quantities and mode of union. Soils contain earths, animal and vegetable remains, saline compounds, and oxide of iron.

The *earths* found in common soils are principally silex or the earth of flints, alumine or the pure matter of clay, lime, or calcareous earth, and magnesia.

Silex, or the earth of flints, when perfectly pure, appears in the form of a white powder, which is incombustible, infusible, Silex.

 insoluble

infoluble in water, and not acted upon by common acids; it is the fubftance which conftitutes the principal part of rock chryftal; it compofes a confiderable part of hard gravelly foils, of hard fandy foils, and of hard ftony lands.

Alumine. *Alumine*, or pure clay, in its perfect ftate is white like filex; it adheres ftrongly to the tongue, is incombuftible, infoluble in water, but foluble in acids, and in fixed alkaline menftrua. It abounds moft in clayey foils and clayey loams; but even in the fmalleft particles of thefe foils it is ufually united to filex and oxide of iron.

Lime. *Lime* is the fubftance well known in its pure ftate under the name of quicklime. It always exifts in foils in combination, and that principally with fixed air or carbonic acid, when it is called carbonate of lime; a fubftance which in the moft compact form conftitutes marble, and in its loofer form chalk. Lime, when combined with fulphuric acid (oil of vitriol), produces fulphate of lime (gypfum), and with phofphoric acid, phofphate of lime. The carbonate of lime, mixed with other fubftances, compofes chalky foils and marles, and it is found in foft fandy foils.

Magnefia. *Magnefia*, when pure, appears as white, and in a lighter powder, than any of the other earths; it is foluble in acid, but not in alkaline menftrua; it is rarely found in foils; when it does exift, it is either in combination with carbonic acid, or with filex and alumine.

Animal decompofing matter. *Animal decompofing matter* exifts in very different ftates, according as the fubftances from which it is produced are different; it contains much carbonaceous fubftance, and may be principally refolved by heat into this fubftance, volatile alkali, inflammable aeriform products, and carbonic acid; it is principally found in lands that have been lately manured.

Vegetable decompofing matter. *Vegetable decompofing matter* is likewife very various in kind, it contains ufually more carbonaceous fubftance than animal matter, and differs from it in the refults of its decompofition principally in not producing volatile alkali; it forms a great proportion of all peats; it abounds in rich mould, and is found in larger or fmaller quantities in all lands.

Saline compounds. The *faline compounds* found in foils are very few, and in quantities fo fmall, that they are rarely to be difcovered. They are principally muriate of foda (common falt), fulphate of magnefia (Epfom falt), and muriate and fulphate of potafh, nitrate of lime, and the mild alkalies.

The

The *oxide of iron* is the fame with the ruft produced by expofing iron to the air and water; it is found in all foils, but is moft abundant in yellow and red clays, and in yellow and red filiceous fands. Oxide of iron.

A more minute account of thefe different fubftances would be incompatible with the object of this paper. A full defcription of their properties and agencies may be found in the elementary books on chemiftry, and particularly in the Syftem of Chemiftry by Dr. Thomfon (2d Ed.); and in Henry's Epitome of Chemiftry.

III. *Inftruments required for the Analyfis of Soils.*

The really important inftruments required for the analyfis of foils are few, and but little expenfive. They are a balance capable of containing a quarter of a pound of common foil, and capable of turning when loaded, with a grain; a feries of weights from a quarter of a pound Troy to a grain; a wire fieve, fufficiently coarfe to admit a pepper corn through its apertures; an Argand lamp and ftand; fome glafs bottles; Heffian crucibles; porcelain, or queen's ware evaporating bafons; a Wedgewood peftle and mortar; fome filters made of half a fheet of blotting paper, folded fo as to contain a pint of liquid, and greafed at the edges; a bone knife, and an apparatus for collecting and meafuring aeriform fluids. Inftruments for analyfis. A balance, weights, fieve, lamp, bottles, crucibles, bafons, p. and mortar, filters, knife, app. for gafes.

The chemical fubftances or reagents required for feparating the conftituent parts of the foil, are muriatic acid (fpirit of falt), fulphuric acid, pure volatile alkali diffolved in water, folution of pruffiate of potafh, foap lye, folution of carbonate of ammoniac, of muriate of ammonia, folution of neutral carbonate of potafh, and nitrate of ammoniac. An account of the nature of thefe bodies, and their effects, may be found in the chemical works already noticed; and the reagents are fold together with the inftruments mentioned above, by Mr. Knight, Fofter Lane, Cheapfide, arranged in an appropriate cheft. Re-agents. Mur. and fulp. acids, vol. alk. pr. potafh, foap lye, carb. amm. mur. amm. carb. pot. nitr. amm.

IV. *Mode of collecting Soils for Analyfis.*

In cafes when the general nature of the foil of a field is to be afcertained, fpecimens of it fhould be taken from different places, two or three inches below the furface, and examined as to the fimilarity of their properties. It fometimes happens, that upon plains the whole of the upper ftratum of the land is How famples of foils are to be collected.

 of

of the ſame kind, and in this caſe, one analyſis will be ſufficient; but in vallies, and near the beds of rivers, there are very great differences, and it now and then occurs that one part of a field is calcareous, and another part filiceous; and in this caſe, and in analogous caſes, the portions different from each other ſhould be ſeparately ſubmitted to experiment.

and preſerved if needful.

Soils when collected, if they cannot be immediately examined, ſhould be preſerved in phials quite filled with them, and cloſed with ground glaſs ſtoppers.

The quantity of ſoil moſt convenient for a perfect analyſis, is from two or four hundred grains. It ſhould be collected in dry weather, and expoſed to the atmoſphere till it becomes dry to the touch.

The ſpecific gravity

The ſpecific gravity of a ſoil, or the relation of its weight to that of water, may be aſcertained by introducing into a phial, which will contain a known quantity of water, equal volumes of water and of ſoil, and this may be eaſily done by pouring in water till it is half full, and then adding the ſoil till the fluid riſes to the mouth; the difference between the weight of the ſoil and that of the water, will give the reſult. Thus if the bottle contains four hundred grains of water, and gains two hundred grains when half filled with water and half with ſoil, the ſpecific gravity of the ſoil will be 2, that is, it will be twice as heavy as water, and if it gained one hundred and ſixty-five grains, its ſpecific gravity would be 1825, water being 1000.

of importance to be known.

It is of importance, that the ſpecific gravity of a ſoil ſhould be known, as it affords an indication of the quantity of animal and vegetable matter it contains; theſe ſubſtances being always moſt abundant in the lighter ſoils.

Other phyſical properties.

The other phyſical properties of ſoils ſhould likewiſe be examined before the analyſis is made, as they denote, to a certain extent, their compoſition, and ſerve as guides in directing the experiments. Thus ſiliceous ſoils are generally rough to the touch, and ſcratch glaſs when rubbed upon it; aluminous ſoils adhere ſtrongly to the tongue, and emit a ſtrong earthy ſmell when breathed on; and calcareous ſoils are ſoft, and much leſs adheſive than aluminous ſoils.

V. Mode

V. *Mode of ascertaining the Quantity of Water of Absorption in Soils.*

Evaporation of the absorbed water, to shew its quantity in soils.

Soils, though as dry as they can be made by continued exposure to air, in all cases still contain a considerable quantity of water, which adheres with great obstinacy to the earths and animal and vegetable matter, and can only be driven off from them by a considerable degree of heat. The first process of analysis is, to free the given weight of soil from as much of this water as possible, without in other respects, affecting its composition; and this may be done by heating it for ten or twelve minutes over an Argand's lamp, in a bason of porcelain, to a temperature equal to 300 * Fahrenheit; and in case a thermometer is not used, the proper degree may be easily ascertained, by keeping a piece of wood in contact with the bottom of the dish; as long as the colour of the wood remains unaltered, the heat is not too high; but when the wood begins to be charred, the process must be stopped. A small quantity of water will perhaps remain in the soil even after this operation, but it always affords useful comparative results; and if a higher temperature were employed, the vegetable or animal matter would undergo decomposition, and in consequence the experiment be wholly unsatisfactory.

One eighth is an extreme proportion.

The loss of weight in the process should be carefully noted, and when in four hundred grains of soil it reaches as high as 50, the soil may be considered as in the greatest degree absorbent, and retentive of water, and will generally be found to contain a large proportion of aluminous earth. When the loss is only from 20 to 10, the land may be considered as only slightly absorbent and retentive, and the silicious earth as most abundant.

VI. *Of the Separation of Stones, Gravel, and vegetable Fibres from Soils.*

Stones, &c. to be separated after the drying, &c.

None of the loose stones, gravel, or large vegetable fibres should be divided from the pure soil till after the water is drawn off; for these bodies are themselves often highly absorbent and retentive, and in consequence influence the fertility of the land.

* In several experiments, in which this process has been carried on by distillation, I have found the water that came over pure, and no sensible quantity of other volatile matter was produced.

The

The next procefs, however, after that of heating, fhould be their feparation, which may be eafily accomplifhed by the fieve, after the foil has been gently bruifed in a mortar. The weights of the vegetable fibres or wood, and of the gravel and ftones fhould be feparately noted down, and the nature of the laft afcertained; if calcareous, they will effervefce with acids; if filiceous, they will be fufficiently hard to fcratch glafs; and if of the common aluminous clafs of ftones, they will be foft, eafily fcratched with a knife, and incapable of effervefcing with acids.

VII. *Separation of the Sand and Clay, or Loam, from each other.*

Sand, clay, and loam feparated from each other by elutriation.

The greater number of foils, befides gravel and ftones, contain larger or fmaller proportions of fand of different degrees of finenefs; and it is a neceffary operation, the next in the procefs of analyfis, to detach them from the parts in a ftate of more minute divifion, fuch as clay, loam, marle, and vegetable and animal matter. This may be effected in a way fufficiently accurate, by agitation of the foil in water. In this cafe, the coarfe fand will generally feparate in a minute, and the finer in two or three minutes, whilft the minutely divided earthy, animal, or vegetable matter will remain in a ftate of mechanical fufpenfion for a much longer time; fo that by pouring the water from the bottom of the veffel, after one, two, or three minutes, the fand will be principally feparated from the other fubftances, which, with the water containing them, muft be poured into a filter, and after the water has paffed through, collected, dried and weighed. The fand muft likewife be weighed, and their refpective quantities noted down. The water of lixiviation muft be preferved, as it will be found to contain the faline matter, and the foluble animal or vegetable matters, if any exift in the foil.

VIII. *Examination of the Sand.*

The fand feparated into filiceous and calcareous.

By the procefs of wafhing and filtration, the foil is feparated into two portions, the moft important of which is generally the finely divided matter. A minute analyfis of the fand is feldom or never neceffary, and its nature may be detected in the fame manner as that of the ftones or gravel. It is always either filicious fand, or calcareous fand, or a mixture of both. If it

confifts

consists wholly of carbonate of lime, it will be rapidly soluble in muriatic acid, with effervescence; but if it consist partly of this substance, and partly of siliceous matter, the respective quantities may be ascertained by weighing the residuum after the action of the acid, which must be applied till the mixture has acquired a sour taste, and has ceased to effervesce. This residuum is the silicious part: it must be washed, dried, and heated strongly in a crucible; the difference between the weight of it and the weight of the whole, indicates the proportion of calcareous sand.

IX. *Examination of the finely divided Matter of Soils, and Mode of detecting mild Lime and Magnesia.*

The finely divided matter of the soil is usually very compound in its nature; it sometimes contains all the four primitive earths of soils, as well as animal and vegetable matter; and to ascertain the proportions of these with tolerable accuracy, is the most difficult part of the subject.

The finely divided matter treated.

The first process to be performed, in this part of the analysis, is the exposure of the fine matter of the soil to the action of the muriatic acid. This substance should be poured upon the earthy matter in an evaporating bason, in a quantity equal to twice the weight of the earthy matter; but diluted with double its volume of water. The mixture should be often stirred, and suffered to remain for an hour or an hour and a half before it is examined.

Muriatic acid takes up lime, magnesia, or iron.

If any carbonate of lime or of magnesia exist in the soil, they will have been dissolved in this time by the acid, which sometimes takes up likewise a little oxide of iron; but very seldom any alumine.

The fluid should be passed through a filter; the solid matter collected, washed with rain water, dried at a moderate heat, and weighed. Its loss will denote the quantity of solid matter taken up. The washings must be added to the solution, which if not sour to the taste, must be made so by the addition of fresh acid, when a little solution of common prussiate of potash must be mixed with the whole. If a blue precipitate occurs, it denotes the presence of oxide of iron, and the solution of the prussiate must be dropped in till no farther effect is produced. To ascertain its quantity, it must be collected in the same manner as other solid precipitates, and heated red; the result is oxide of iron.

Precip. of iron (if present) by pruss. alkali;

Into

and earth, by carbonate of potash.

Into the fluid freed from oxide of iron, a solution of neutralized carbonate of potash must be poured till all effervescence ceases in it, and till its taste and smell indicate a considerable excess of alkaline salt.

The precipitate that falls down is carbonate of lime; it must be collected on the filter, and dried at a heat below that of redness.

The remaining fluid must be boiled for a quarter of an hour, when the magnesia, if any exist, will be precipitated from it, combined with carbonic acid, and its quantity is to be ascertained in the same manner as that of the carbonate of lime.

Alumine if taken up.

If any minute proportion of alumine should, from peculiar circumstances, be dissolved by the acid, it will be found in the precipitate with the carbonate of lime, and it may be separated from it by boiling for a few minutes with soap lye, sufficient to cover the solid matter. This substance dissolves alumine, without acting upon carbonate of lime.

Carbonate of lime if in plenty, may be estimated by the quantity of carbonic acid.

Should the finely divided soil be sufficiently calcareous to effervesce very strongly with acids, a very simple method may be adopted for ascertaining the quantity of carbonate of lime, and one sufficiently accurate in all common cases.

Carbonate of lime, in all its states, contains a determinate proportion of carbonic acid, *i. e.* about 45 per cent. so that when the quantity of this elastic fluid, given out by any soil during the solution of its calcareous matter in an acid is known, either in weight or measure, the quantity of carbonate of lime may be easily discovered.

When the process by diminution of weight is employed, two parts of the acid and one part of the matter of the soil must be weighed in two separate bottles, and very slowly mixed together till the effervescence ceases; the difference between their weight before and after the experiment, denotes the quantity of carbonic acid lost; for every four grains and a half of which, ten grains of carbonate of lime must be estimated.

The best method of collecting the carbonic acid, so as to discover its volume, is by the pneumatic apparatus, the construction and application of which is described at the end of this paper. The estimation is, for every ounce measure of carbonic acid, two grains of carbonate of lime.

X. Mode

X. *Mode of ascertaining the Quantity of insoluble finely divided Animal and Vegetable Matter.*

After the fine matter of the soil has been acted upon by muriatic acid, the next process is to ascertain the quantity of finely divided insoluble animal and vegetable matter that it contains.

Ignition in an open vessel destroys vegetable and animal matters.

This may be done with sufficient precision, by heating it to strong ignition in a crucible over a common fire till no blackness remains in the mass. It should be often stirred with a metallic wire, so as to expose new surfaces continually to the air; the loss of weight that it undergoes denotes the quantity of the substance that it contains destructible by fire and air.

It is not possible to ascertain whether this substance is wholly animal or vegetable matter, or a mixture of both. When the smell emitted during the incineration is similar to that of burnt feathers, it is a certain indication of some animal matter; and a copious blue flame at the time of ignition, almost always denotes a considerable proportion of vegetable matter. In cases when the experiment is needed to be very quickly performed, the destruction of the decomposible substances may be assisted by the agency of nitrate of ammoniac, which at the time of ignition may be thrown gradually upon the heated mass in the quantity of twenty grains for every hundred of residual soil. It affords the principle necessary to the combustion of the animal and vegetable matter, which it causes to be converted into elastic fluids; and it is itself at the same time decomposed and lost.

The smell shews whether it be animal or vegetable.

XI. *Mode of separating aluminous and silicious Matter and Oxide of Iron.*

The substances remaining after the decomposition of the vegetable and animal matter, are generally minute particles of earthy matter, containing usually alumine and silex with combined oxide of iron.

The residual silex, alumine and oxide of iron separated.

To separate these from each other, the solid matter should be boiled for two or three hours with sulphuric acid, diluted with four times its weight of water; the quantity of the acid should be regulated by the quantity of solid residuum to be acted on, allowing for every hundred grains two drachms or one hundred and twenty grains of acid.

Dilute sulphuric acid takes up the two first.

The

The ſubſtance remaining after the action of the acid, may be conſidered as ſilicious; and it muſt be ſeparated and its weight aſcertained, after waſhing and drying in the uſual manner.

Carbonate of ammonia throws down the alumine.

The alumine and the oxide of iron, if they exiſt, are both diſſolved by the ſulphuric acid; they may be ſeparated by carbonate of ammoniac, added to exceſs; it throws down the alumine, and leaves the oxide of iron in ſolution, and this ſubſtance may be ſeparated from the liquid by boiling.

Should any magneſia and lime have eſcaped ſolution in the muriatic acid, they will be found in the ſulphuric acid; this, however, is ſcarcely ever the caſe; but the proceſs for detecting them and aſcertaining their quantities, is the ſame in both inſtances.

More accurate proceſs.

The method of analyſis by ſulphuric acid, is ſufficiently preciſe for all uſual experiments; but if very great accuracy be an object, dry carbonate of potaſh muſt be employed as the agent, and the reſiduum of the incineration muſt be heated red for half an hour, with four times its weight of this ſubſtance, in a crucible of ſilver, or of well baked porcelain. The maſs obtained muſt be diſſolved in muriatic acid, and the ſolution evaporated till it is nearly ſolid; diſtilled water muſt then be added, by which the oxide of iron and all the earths, except ſilex, will be diſſolved in combination as muriates. The ſilex, after the uſual proceſs of lixiviation, muſt be heated red; the other ſubſtances may be ſeparated in the ſame manner as from the muriatic and ſulphuric ſolutions.

This proceſs is the one uſually employed by chemical philoſophers for the analyſis of ſtones.

XII. *Mode of diſcovering ſoluble Animal and Vegetable Matter, and Saline Matter.*

Matters ſoluble in water.

If any ſaline matter, or ſoluble vegetable or animal matter, is ſuſpected in the ſoil, it will be found in the water of lixiviation uſed for ſeparating the ſand.

This water muſt be evaporated to dryneſs in an appropriate diſh, at a heat below its boiling point.

If the ſolid matter obtained is of a brown colour and inflammable, it may be conſidered as partly vegetable extract. If its ſmell, when expoſed to heat, be ſtrong and fetid, it contains animal mucilaginous or gelatinous ſubſtance; if it be white

white and transparent, it may be considered as principally saline matter. Nitrate of potash (nitre) or nitrate of lime, is indicated in this saline matter, by its scintillating with a burning coal. Sulphate of magnesia may be detected by its bitter taste; and sulphate of potash produces no alteration in solution of carbonate of ammoniac, but precipitates solution of muriate of barytes.

XIII. *Mode of detecting Sulphate of Lime (Gypsum) and Phosphate of Lime in Soils.*

Should sulphate of phosphate of lime be suspected in the entire soil, the detection of them requires a particular process upon it. A given weight of it, for instance four hundred grains, must be heated red for half an hour in a crucible, mixed with one-third of powdered charcoal. The mixture must be boiled for a quarter of an hour, in a half pint of water, and the fluid collected through the filter, and exposed for some days to the atmosphere in an open vessel. If any soluble quantity of sulphate of lime (gypsum) existed in the soil, a white precipitate will gradually form in the fluid, and the weight of it will indicate the proportion. Sulphate of Lime.

Phosphate of lime, if any exist, may be separated from the soil after the process for gypsum. Muriatic acid must be digested upon the soil, in quantity more than sufficient to saturate the soluble earths; the solution must be evaporated, and water poured upon the solid matter. This fluid will dissolve the compounds of earths with the muriatic acid, and leave the phosphate of lime untouched. Phosphate of lime.

It would not fall within the limits assigned to this paper, to detail any processes for the detection of substances which may be accidentally mixed with the matters of soils. Manganese is now and then found in them, and compounds of the barytic earth; but these bodies appear to bear little relation to fertility or barrenness, and the search for them would make the analysis much more complicated without rendering it more useful.

XIV. *Statement of Results and Products.*

When the examination of a soil is compleated, the products should be classed, and their quantities added together, and if they nearly equal the original quantity of soil, the analysis may be considered as accurate. It must, however, be noticed, Products stated.

 that

that when phosphate or sulphate of lime are discovered by the independent process XIII. a correction must be made for the general process, by subtracting a sum equal to their weight from the quantity of carbonate of lime, obtained by precipitation from the muriatic acid.

In arranging the products, the form should be in the order of the experiments by which they were obtained.

Thus 400 grains of a good silicious sandy soil may be supposed to contain

		Grains.
Of water of absorption - - - - -		18
Of loose stones and gravel principally silicious -		42
Of undecompounded vegetable fibres - -		10
Of fine silicious sand - - -		200
Of minutely divided matter separated by filtration and consisting of		
Carbonate of lime - - -	25	
Carbonate of magnesia - - -	4	
Matter destructible by heat, principally vegetable - - - -	10	
Silex - - - - -	40	
Alumine - - - - -	32	
Oxide of iron - - - -	4	
Soluble matter, principally sulphate of potash and vegetable extract - -	5	
Gypsum - - - - -	3	
Phosphate of lime - - -	2	
	———	125
Amount of all the products		395
Loss - - - -		5

In this instance the loss is supposed small; but in general, in actual experiments, it will be found much greater, in consequence of the difficulty of collecting the whole quantities of the different precipitates; and when it is within thirty for four hundred grains, there is no reason to suspect any want of due precision in the processes.

XV. *This general Method of Analysis may in many Cases be much simplified.*

Simplification, &c. of the analysis.

When the experimenter is become acquainted with the use of the different instruments, the properties of the reagents, and

and the relations between the external and chemical qualities of ſoils, he will ſeldom find it neceſſary to perform, in any one caſe, all the proceſſes that have been deſcribed. When his ſoil, for inſtance, contains no notable proportion of calcareous matter, the action of the muriatic acid IX. may be omitted. In examining peat ſoils, he will principally have to attend to the operation by fire and air X.; and in the analyſis of chalks and loams, he will often be able to omit the experiment by ſulphuric acid XI.

In the firſt trials that are made by perſons unacquainted with chemiſtry, they muſt not expect much preciſion of reſult. Many difficulties will be met with; but in overcoming them, the moſt uſeful kind of practical knowledge will be obtained; and nothing is ſo inſtructive in experimental ſcience, as the detection of miſtakes. The correct analyſt ought to be well grounded in chemical information; but perhaps there is no better mode of gaining it, than that of attempting original inveſtigations. In purſuing his experiments, he will be continually obliged to learn from books, the hiſtory of the ſubſtances he is employing or acting upon; and his theoretical ideas will be more valuable in being connected with practical operation, and acquired for the purpoſe of diſcovery.

XVI. *On the Improvement of Soils, as connected with the Principle of their Compoſition.*

Improvement of lands from the known compoſition of fertile or ſterile ſoils.

In caſes when a barren ſoil is examined with a view to its improvement, it ought in all caſes, if poſſible, to be compared with an extremely fertile ſoil in the ſame neighbourhood, and in a ſimilar ſituation: the difference given by their analyſes would indicate the methods of cultivation; and thus the plan of improvement would be founded upon accurate ſcientific principles.

If the fertile ſoil contained a large quantity of ſand, in proportion to the barren ſoil, the proceſs of amelioration would depend ſimply upon a ſupply of this ſubſtance; and the method would be equally ſimple with regard to ſoils deficient in clay or calcareous matter.

In the application of clay, ſand, loam, marle, or chalk to lands, there are no particular chemical principles to be obſerved; but when quick lime is uſed, great care muſt be taken that it is not obtained from the magneſian limeſtone; for in this

this caſe, as has been ſhewn by Mr. Tennant, it is exceedingly injurious to land *. The magneſian limeſtone may be diſtinguiſhed from the common limeſtone by its greater hardneſs, and by the length of time that it requires for its ſolution in acids, and it may be analyſed by the proceſs for carbonate of lime and magneſia IX.

When the analytical compariſon indicates an exceſs of vegetable matter, as the cauſe of ſterility, it may be deſtroyed by much pulverization and expoſure to air, by paring and burning, or the agency of lately made quicklime. And the defect of animal and vegetable matter muſt be ſupplied by animal or vegetable manure.

XVII. *Sterile Soils in different Climates and Situations muſt differ in Compoſition.*

Different climates and local circumſtances require different compounds for fertile ſoils.

The general indications of fertility and barrenneſs, as found by chemical experiments, muſt neceſſarily differ in different climates, and under different circumſtances. The power of ſoils to abſorb moiſture, a principle eſſential to their productiveneſs, ought to be much greater in warm and dry countries, than in cold and moiſt ones; and the quantity of fine aluminous earth they contain larger. Soils likewiſe that are ſituated on declivities, ought to be more abſorbent than thoſe in the ſame climate on plains or in valleys †. The productiveneſs of ſoils muſt likewiſe be influenced by the nature of the ſubſoil, or the earthy or ſtony ſtrata on which they reſt; and this circumſtance ought to be particularly attended to, in conſidering their chemical nature, and the ſyſtem of improvement. Thus a ſandy ſoil may ſometimes owe its fertility to the power of the ſubſoil to retain water; and an abſorbent clayey ſoil may occaſionally be prevented from being barren, in a moiſt climate, by the influence of a ſubſtratum of ſand or gravel.

XVIII. *Of the chemical Compoſition of fertile Corn Soils in this Climate.*

Actual compoſition of ſome fertile ſoils.

Thoſe ſoils that are moſt productive of corn, contain always certain proportions of aluminous and calcareous earth in a finely divided ſtate, and a certain quantity of vegetable or animal matter.

* Phil. Tranſactions for 1799, p. 305. This limeſtone is found abundantly in Yorkſhire, Derbyſhire, and Somerſetſhire.

† Kirwan. Tranſ. Iriſh Academy, Vol. V. p. 17[illegible].

The

The quantity of calcareous earth is however very various, and in ſome caſes exceedingly ſmall. A very fertile corn ſoil from Ormiſton in Eaſt Lothian afforded me in an hundred parts, only eleven parts of mild calcareous earth; it contained twenty-five parts of ſilicious ſand; the finely divided clay amounted to forty-five parts. It loſt nine in decompoſed animal and vegetable matter, and four in water, and afforded indications of a ſmall quantity of phoſphate of lime.

This ſoil was of a very fine texture, and contained very few ſtones or vegetable fibres. It is not unlikely that its fertility was in ſome meaſure connected with the phoſphate; for this ſubſtance is found in wheat, oats, and barley, and may be a part of their food.

A ſoil from the low lands of Somerſetſhire, celebrated for producing excellent crops of wheat and beans without manure, I found to conſiſt of one-ninth of ſand, chiefly ſilicious, and eight-ninths of calcareous marle tinged with iron, and containing about five parts in the hundred of vegetable matter. I could not detect in it any phoſphate or ſulphate of lime, ſo that its fertility muſt have depended principally upon its power of attracting principles of vegetable nouriſhment from water and the atmoſphere *.

Mr. Tillet, in ſome experiments made on the compoſition of ſoils at Paris, found that a ſoil compoſed of three-eighths of clay, two-eighths of river ſand, and three-eighths of the parings of limeſtone, was very proper for wheat.

XIX. *Of the Compoſition of Soils proper for bulbous Roots and for Trees.*

In general, bulbous roots require a ſoil much more ſandy, and leſs abſorbent than the graſſes. A very good potatoe ſoil, from Varſel in Cornwall, afforded me ſeven-eighths of ſilicious ſand; and its abſorbent power was ſo ſmall, that one hundred parts loſt only two by drying at 400 Fahrenheit. Soils proper for bulbous roots and trees.

Plants and trees, the roots of which are fibrous and hard, and capable of penetrating deep into the earth, will vegetate to advantage in almoſt all common ſoils, which are moderately dry, and which do not contain a very great exceſs of vegetable matter.

* This ſoil was ſent to me by T. Poole, Eſq. of Nether Stowey. It is near the opening of the river Parret into the Britiſh Channel; but, I am told, is never overflowed.

I found

I found the soil taken from a field at Sheffield-place in Sussex, remarkable for producing flourishing oaks, to consist of six parts of sand, and one part of clay and finely divided matter. And one hundred parts of the entire soil submitted to analysis, produced

	Parts.
Water - - - - - -	3
Silex - - - - - -	54
Alumine - - - - - -	28
Carbonate of lime - - -	3
Oxide of iron - - - - -	5
Decomposing vegetable matter -	4
Loss - - - - - - -	3

XX. *Advantages of Improvements made by changing the Composition of the earthy Parts of Soils.*

Soils rendered fertile by changing the composition of the earthy parts, are more permanent than manured soils.

From the great difference of the causes that influence the productiveness of lands, it is obvious that in the present state of science, no certain system can be devised from their improvement, independent of experiment; but there are few cases in which the labour of analytical trials will not be amply repaid by the certainty with which they denote the best methods of amelioration; and this will particularly happen, when the defect of composition is found in the proportions of the primitive earths.

In supplying animal or vegetable manure, a temporary food only is provided for plants, which is in all cases exhausted by means of a certain number of crops; but when a soil is rendered of the best possible constitution and texture, with regard to its earthy parts, its fertility may be considered as permanently established. It becomes capable of attracting a very large portion of vegetable nourishment from the atmosphere, and of producing its crops with comparatively little labour and expence.

Description of the Apparatus for the Analysis of Soils.

Apparatus for experiments.

A. Retort.

B. B. Funnels for the purpose of filtrating.

D. Balance.

E. Argand's lamp.

F, G, H, K. The different parts of the apparatus required for measuring the quantity of elastic fluid given out during the

the action of an acid on calcareous soils. F. Represents the bottle for containing the soil. K. The bottle containing the acid furnished with a stopcock. G. The tube connected with a flaccid bladder. I. The graduated measure. H. The bottle for containing the bladder. When this instrument is used, a given quantity of soil is introduced into F; K is filled with muriatic acid diluted with an equal quantity of water; and the stop-cock being closed is connected with the upper orifice of F, whieh is ground to receive it. The tube G is introduced into the lower orifice of F, and the bladder connected with it placed in its flaccid state into H, which is filled with water. The graduated measure is placed under the tube of H. When the stop-cock of K is turned, the acid flows into F, and acts upon the soil; the elastic fluid generated passes through G into the bladder, and displaces a quantity of water in H equal to it in bulk, and this water flows through the tube into the graduated measure; the water in which gives by its volume the indication of the proportion of carbonic acid disengaged from the soil; for every ounce measure of which two grains of carbonate of lime may be estimated.

L. Represents the stand for the lamp.

M, N, O, P, Q, R, S. Represent the bottles containing the different reagents.

IV.

Discovery of a new Vegetable Substance, by Mr. Rose *.

Decoction of elecampane root deposits a powder resembling starch.

A CONCENTRATED decoction of the root of elecampane, *inula helenium*, after standing some hours, deposits a white powder, appearing at first sight much like starch, but differing from it both in its principles and in its manner of comporting itself with other substances.

It is insoluble in cold water.

1. This substance is generally insoluble in cold water. Being triturated with it a white milky liquor is formed, which soon deposits a heavy white powder, and leaves the supernatant water clear and limpid.

Soluble in boiling water.

2. It dissolves very well in boiling water. On heating to ebullition one part of the white powder, with four parts of water,

* From Gehlen's Journal for 1804, Vol. III. p. 217.

water, a complete solution is obtained, which passes through filtering paper while hot, but on cooling acquires a mucilaginous consistence and a dull colour. In the course of some hours this solution deposits the greater part of the substance dissolved in the form of a compact white powder.

But much subsides on cooling.

A solution of one part of gum arabic, in four parts of water is much thicker, of a more tenacious consistence, and froths lightly, which the solution of the powder from the elecampane root does not.

Differs from solution of gum arabic.

3. On mixing the solution of the white powder with an equal quantity of alcohol, the mixture is at first clear, but in a little time the powder separates in the form of a tumid white sediment, leaving the fluid above it transparent. A solution of gum-arabic on the addition of alcohol becomes immediately milky, and long retains this appearance, no kind of powder separating even in several days.

Alcohol separates it from water,

does not gum-arabic.

4. When thrown on burning coals, the white powder melts like sugar and evaporates, diffusing a white, thick, pungent smoke, with a smell of burnt sugar. After this combustion a light residuum only remains, which runs into the coal. Starch emits a similar smoke, but does not melt, and leaves a coally residuum much greater in quantity. Gum-arabic under the same circumstances gives out scarcely any smoke.

Melts, emits a thick smoke, and leaves little residuum.

Thus differs from starch,

and from gum.

Heated in an iron spoon over charcoal the powder first melts, and emits the smoke above described. As soon as the spoon becomes red hot, it burns with a vivid light flame, and leaves a very trifling coally residuum. Starch under the same circumstances does not melt, is much longer before it burns, and leaves a considerable residuum of coally matter. Gum-arabic only sparkles, does not take fire, and leaves a great deal of coal, which is readily convertible into grayish ashes.

On red hot iron burns.

Starch.

5. By dry distillation we obtain from this powder of the elecampane root a brown empyreumatic acid, having the smell of pyroxalic acid, but not an atom of empyreumatic oil.

Dry distillation produces an acid, but no oil.

6. The nitric acid transforms the powder only into malic acid and oxalic acid, and when used in great excess into acetic acid: but we do not obtain an atom of the saccholactic acid, which gum-arabic treated in the same manner furnishes so abundantly; or of the fatty matter which is generated by the action of nitric acid on starch.

Nitric acid produces malic, oxalic, and in excess acetic.

Gum the saccholactic acid.

Starch fat.

2 From

From all these phenomena it follows, that this farinaceous powder extracted from elecampane root, is neither starch nor gum, but a peculiar vegetable substance holding a middle rank between the two. It is probable, that it exists in many other vegetables, and that several products hitherto considered as starch are of the same nature as this farina. Hence of a nature between gum and starch, and probably exists in other vegetables.

V.

New Galvanic Discoveries by Mr. RITTER, *extracted from a Letter from Mr.* CHRIST. BERNOULLI *.

I HERE transmit you the information you requested respecting the late experiments of Mr. Ritter, to which I subjoin some account of that gentleman.

1. *Charging of a Louis d'Or by the Pile.*

The pile with which Mr. Ritter usually makes his experiments consists of a hundred pairs of metallic plates, two inches in diameter. The pieces of zinc have a rim to prevent the liquid pressed out from flowing away. The apparatus is always insulated by several plates of glass. Mr. Ritter's common pile.

As Mr. Ritter at present resides in a village near Jene, I have not been able to see his experiments with his grand battery of two thousand pieces, or with his battery of fifty pieces, each thirty-six inches square, the action of which continues very perceptible for a fortnight. Neither have I seen his experiments with the new battery of his invention, consisting of a single metal, and which he calls *the charging pile.* His grand battery, large battery, and charging pile.

I have frequently however, seen him galvanise louis d'or lent him by persons present. To effect this, he places the louis between two pieces of pasteboard thoroughly wetted, and keeps it six or eight minutes in the chain of circulation connected with the pile. Thus the louis becomes charged, without being immediately in contact with the conducting wires. If this louis be applied afterward to the crural nerves of a frog recently prepared, the usual contractions will be excited. Louis d'or charged by being kept in the galvanic circuit, excites contractions,

* Translated from the *Journal de Chimie and de Physique* of Van Mons, No. 17, p. 133, March, 1805.

and may thus be distinguished among others, excited. I had put a louis thus galvanifed into my pocket, and Mr. Ritter faid to me a few minutes after, that I might find out this louis from among the reft, by trying them in fucceffion upon the frog. Accordingly I made the trial, and in reality diftinguifhed among feveral others a fingle one, in which the exciting quality was very evident. This charge is retained in proportion to the time that the piece has remained in the circuit of the pile. Of three different louis which Mr. Ritter charged in my prefence, neither loft its charge in lefs than five minutes. All thefe experiments fucceeded completely, and nothing feemed fo eafy as to repeat them.

as it does not lofe its charge for fome minutes.

This fhows the affinity of the galvanic with the magnetic fluid, between which and the electric, it holds a middle place.

This retention of the galvanic charge by a metal in contact with the hand, and with other metals, fhews this communication of the galvanic virtue to have more affinity with magnetifm than with electricity, and affigns to the galvanic fluid an intermediate rank between the other two.

Several pieces may be charged at once.

In the manner which I have juft defcribed, Mr. Ritter can charge at once as many pieces as he wifhes. It is fufficient if the two extreme pieces of the number communicate with the pile through the intervention of wet pafteboards. It is with metallic difcs charged in this manner, and placed upon one another with pieces of wet pafteboard alternately interpofed, that Mr. Ritter conftructs his charging pile, which ought in remembrance of its inventor to be called the *Ritterian pile*. The conftruction of this pile fhows, that each metal galvanifed in this way acquires polarity, as the needle does when touched with a magnet. Though I have had no opportunity of feeing this new pile, I have convinced myfelf of the reality of the phenomenon by an experiment of the higheft importance to fcience, and for the invention of which we are equally indebted to the fame ingenious philofopher.

Ritterian pile.

Metals thus charged acquire polarity.

2. *Different Excitability of the Parts of Animals.*

Different excitability of the parts of animals.

During the courfe of feveral years in which Mr. Ritter has been employed in galvanic purfuits, and during which he has made many thoufands of experiments on the excitation produced in the frog by the contact of two different metals, for Mr. Ritter has not entirely abandoned the original mode of galvanifing, like moft other experimentalifts, who employ Volta's pile exclufively; he had perceived not only a very ftriking difference in the excitability of the different parts of animals,

animals, but alſo a difference of excitement between the extenſor and flexor muſcles, according as the poſitive or negative pole was applied to them, or as they were acted upon the inſtant after the metals were brought into contact or ſeparated from each other.

When the excitability is at its higheſt point of energy, as in very young frogs the moment after they are prepared, or in adult frogs during the coupling ſeaſon, the flexors alone contract, and in particular the flexor muſcles of that thigh to which the ſilver or negative metal is applied, contract at the inſtant when the metals come into contact, while thoſe of the thigh to which the zinc or poſitive metal is applied, contract at the inſtant of their ſeparation.

When the excitability of the animal is greateſt, the flexors contract by poſitive galvaniſm:

The oppoſite effects are obſervable in frogs, the excitability of which is on the point of being extinguiſhed, (Ritter's fifth degree.) In this caſe the extenſors only contract, and the flexors remain abſolutely motionleſs. At the moment of contact of the metals the muſcles on the zinc ſide alone are thrown into action, and at the moment of ſeparation thoſe on the ſilver ſide.

when it is loweſt the extenſors contract by negative.

Mr. Ritter diſtinguiſhes three degrees of mean excitability. At the ſecond degree (the firſt of the three mean degrees,) when the metals are brought into contact, a ſtrong excitement of the flexors is diſplayed on the ſilver ſide, and a weak excitement of the extenſors on the zinc ſide; and when the metals are ſeparated a ſtrong excitement of the flexors is ſeen on the zinc ſide, and a weak excitement of the extenſors on the ſilver ſide.

When the excitability is between the medium and either extreme, the effect on the flexors and extenſors ſimultaneous but unequal.

At the fourth degree of excitability the contrary takes place. At the third or middle degree the excitability appears to be equally diſtributed, the contractions on each ſide appear equal, and at the moment of contact the flexors contract on the ſilver ſide, the extenſors on the zinc ſide; while at the moment of ſeparation the extenſors contract on the ſilver ſide, and the flexors on the zinc ſide.

At the medium, equal as well as ſimultaneous.

Mr. Ritter ſhowed me all theſe phenomena, and it was very eaſy to diſtinguiſh the different contractions. I have not yet had time to repeat theſe experiments, but I am afraid, eaſy as they appeared to be, they will require an experienced hand, to produce ſuch diſtinct effects as I ſaw. None of the experiments which Mr. Ritter performed before me ſucceeded with him

The experiments do not always ſucceed on the firſt trial.

Mr. Ritter's merit not sufficiently appreciated,

him the first time. Most of these experiments have never yet been made public, and few philosophers have justly appreciated the value of those which have been given to the world. There are some people, who, habituated solely to the striking effects of grosser physics, suppose it impossible for a young philosopher to see any thing more than themselves in the delicate phenomena of a more refined order of physical experiments. What has greatly contributed to prevent Mr. Ritter from attaining the high reputation he deserves is his style, which, by endeavouring to give it preeision, he has rendered obscure; but in conversation it is quite otherwise, as here he combines the strictest logic with the greatest simplicity of expression.

partly owing to his style.

Account of Mr. Ritter.

Mr. Ritter is one of those men, who owe every thing to the inspiration of genius, nothing to education. He was intended for a mechanical occupation, when the discoveries of galvani excited in him that innate taste for the physical sciences, which has carried him over every obstacle, and raised him to rank among the first natural philosophers. Destitute of every source for procuring himself the apparatus indispensable to ordinary physics, but swayed by the enthusiasm of inquiry, he greedily seized the opportunity of obeying this impulse by pursuing a series of experiments, that require only a simple and not a very expensive apparatus. Europe has rung with the success he has obtained within the seven years he has given to his researches. He must have written much to procure himself a large pile, and the most necessary books of natural philosophy.

Not less indefatigable as an experimenter than ingenious as a theorist, he has committed to writing thousands of experiments, which his time divided between galvanic experiments, application to other branches of physics, and the study of languages, has not yet allowed him to put in order for publication. But this state of constraint is about to be at an end. The elector Bavaria, that enlightened prince, whose philosophical beneficence attracts to his dominions the most distinguished men of science and learning throughout Europe, has just appointed Mr. Ritter a Member of the Academy of Munich, with a salary of about 200l. a year.

He is composing a systematic work on galvanism.

Mr. Ritter has been employed these six months in composing a systematic work on galvanism, but he does not think

he

he shall be able to finish it in less than two or three years. When I left him he was going to publish *Tables of Galvanic Affinity*, including all the substances on which he has made experiments. These tables will be of as much importance to galvanism as those of Bergman were to chemistry: they will show, though not yet in a complete manner, the order in which substances follow each other with respect to exciting or receiving the galvanic action. Publishing tables of galvanic affinity.

But to return to the experiments respecting the charging of metals. Mr. Ritter, after having shown me his experiments on the different contractibility of various muscles, made me observe, that the piece of gold galvanised by communication exerts at once the action of two metals, or of one constituent part of the pile; and that the half which was next the negative pole while in the circle became positive, and the half toward the positive pole became negative. I was completely convinced of the reality of these different phenomena, so important to physic in general, and to physiology in particular. The galvanised piece of metal has two poles.

Mr. Ritter having discovered the method of galvanising metals, as iron is rendered magnetic, and having observed that galvanised metals always exhibit two poles, as the magnetic needle does, had the curiosity to observe the effect of golden needles charged with galvanism and balanced on a pivot. To his surprise he perceived, that these needles had a certain dip and variation, and that the angle of variation, the quantity of which I am sorry I cannot recollect, was uniformly the same in all his experiments. It differs however from that of the magnetic needle, and the positive pole always dips. Golden needles galvanised and suspended, have both dip and variation, but different from the magnetic.

VI.

Improvement in applying the Points in Electrical Machines. By Mr. G. J. Singer.

To Mr. NICHOLSON.

SIR, *Princes Street, Sep. 19th*, 1805.

IN the ordinary construction of electrical machines, the collecting points are fixed, and by the least accidental motion are liable to scratch the glass, to obviate this inconvenience, I place

place my points in a cylindrical wire, terminated by smooth wooden balls, whose semidiameter is less than the length of the points: This wire is moveable on its axis, by means of a spring socket annexed to the stem which enters the conductor: The points may of course be placed at any required elevation, and the greatest intensity any variation in their situation produces, be obtained. When the points are elevated a little above the horizontal line, the danger of scratching the glass is effectually prevented, by the balls coming in contact while the points are kept at a small distance. The security this application produces, and the additional intensity it affords, have induced me to trouble you with this communication.

I am,
Dear Sir,
Your's, &c.
G. J. SINGER.

VII.

Question whether Light as a Body may not have its Temperature raised or lowered, and produce the Effects ascribed to reflected Heat. By J. P.

To Mr. NICHOLSON.

SIR,

Question respecting light.

POSSESSING no differential thermometer, nor any time to employ it, I cannot prove whether my opinion is well founded or not, respecting the ingenious experiments of Mr. Leslie or of M. Pictet, by which the reflection of invisible (not radiant) heat, and even of cold, appears to have been proved.

Instead of there being an actual reflection of heat as a substance, or of cold as a substance, is it not in all these cases a reflection of heated or of cooled light? In the experiments with the heated cannister, the light of the room is, I doubt not, heated by the cannister; and if collected in a focus, must produce an effect on the thermometer, answerable to the increased quantity of heat with which it is impregnated. Thus also in Monf. Pictet's experiment, the light intercepted by the mirror and thence reflected, has been deprived of a portion of its caloric, or in other words cooled, by the ice; at the focal

focal point therefore will be a collection of cooled rays of light, which must necessarily occasion an effect on the thermometer, the reverse of that of the former experiment. That light is a body capable of being united with caloric, and that heated or cooled light should thus be reflected and occasion all the phenomena of Mr. Leslie's and of M. Pictet's experiments, appears to be much more probable, than that this calorific and frigorific fluid should be the ambient air, or that cold, as a body, should be reflected from mirrors in such a manner as light is perhaps only capable of being reflected. Were the experiments so made that no light should be in the room, and only a small confined portion of light used to examine the thermometer, these conjectures would be put to the trial, and I trust the mystery would be removed.

Sir, your's,

J. P.

VIII.

*Experiments on a Mineral formerly called false Tungstein, now Cerite, in which a new Metal has been found.**

MR. Klaproth, about eight months ago, says Mr. Vauquelin, sent me word, that he had discovered in the tungstein of Bastnas a new earth, to which he gave the name of ochroit, on account of the red colour it acquired by calcination. Messrs. Hisinger and Berzelius, hearing this, wrote to Mr. V. claiming the priority of discovery, but affirming at the same time, that what they had found was a new metal. These gentlemen afterward sent Mr. V. specimens of the mineral, which he analysed in company with two experienced practical chemists, Messrs. Tassaert and Bergman. The following were the results of their analysis: (Klaproth supposed he had discovered a new earth in the false tungstein; Hisinger and Berzelius a new metal.)

The pure cerite † is semitransparent, with a slight rosy tinge, (Characters of pure cerite.)

* Abridged from a paper by Vauquelin in the *Annales de Chimie*, Vol. LIV. p. 28, and another by Messrs. Hisinger and Berzelius in van Mons's *Journal de Chimie*, Vol. VI. p. 142.—C.

† Messrs. H. and B. have given to the metal the name of *cerion* or *cerium*, from the new planet Ceres, and to the mineral in which they discovered it that of *cerite*.

or

or of a light or deep flesh-colour *. It is sufficiently hard to scratch glass †, strikes fire with difficulty, and its specific gravity is 4·530. It has no determinate crystalline figure. Its fracture is compact ‡, and a little shining. Its powder is of a greyish colour; it becomes yellow by calcination, and loses twelve per cent §.

Treated with nitro-muriatic acid.

Exp. 1. Two hundred parts of this mineral treated with nitro-muriatic acid three times successively, gave abundance of nitrous acid and oxigenated muriatic acid gas. The first and second solutions being diluted with water were of a gold colour; the third was colourless. The former two being mixed deposited spontaneously in time a small quantity of white sediment. The residuum left by the nitro-muriatic acid was of a gray colour with a slight roseate tinge, and weighed 62, so that 138 parts were dissolved.

The solution precipitated

Exp. 2. The solutions being evaporated to the consistence of syrup to volatilise, the superfluous acid remained clear to the end of the operation Their residuum, diluted with water, afforded a milky liquor, with a slight rosy tint, and a very astringent taste.

by prussiate of potash and ammonia.

Prussiate of potash produced in it a greenish blue precipitate: the colour of which was changed to a brown by a small quantity of ammonia.

All the liquor into which a small quantity of ammonia had been put to precipitate the iron alone was poured into a filter, but would not pass through. It was heated therefore, and filtered, when it appeared of a gold colour, and had a very saccharine taste. Prussiate of potash and oxalate of ammonia threw down from it perfectly white precipitates.

The matter left on the filter continued for a long time to impart a yellow tinge to the water with which it was washed. It was of a red colour, and appeared like oxide of iron at a maximum of oxidation.

Examined by different reagents.

The solution thus deprived of the red matter by ammonia, was examined by various reagents. Prussiate of potash gave with it a white, flocculent, gelatinous precipitate. Infusion

* Opake, and sometimes but very rarely, yellowish. Messrs. H. and B.

† Does not scratch glass. H. and B.

‡ Unequal and angular. H. and B.

§ Six or seven. H. and B.

of

of galls, a brown, flocculent sediment, unaffected by muriatic acid. Carbonate of potash, a very copious white gelatinous precipitate. Caustic potash, the same: and an excess of this reagent produced no change. Oxalate of ammonia, a very copious, white, flocculent precipitate, insoluble in an excess of oxalic acid. Sulphuric acid, a yellow crystalline precipitate soluble in water. Muriate of tin whitened the solution without forming any precipitate.

Exp. 3. After this the solution was evaporated, when it instantly became turbid, and formed an abundant flesh-coloured deposit. This was treated with acidulous oxalate of potash to dissolve the iron without success: the addition of nitric acid was as unsuccessful: but muriatic acid added to the preceding dissolved the precipitate with effervescence and the emission of oxigenated muriatic acid gas. A white crystalline substance however, remained, consisting of oxide of cerium with oxalic acid. The greater part of the excess of acid in the solution being saturated with ammonia, oxalate of ammonia was added till no more precipitate was formed. This precipitate had all the properties of oxalate of cerium. Ammonia threw down from the filtered liquor oxide of iron. Attempts to free it from iron.

Exp. 4. The matter precipitated from the solution of cerium by ammonia in *Exp.* 2, dissolved with effervescence in muriatic acid. Oxalate of ammonia threw down from this solution oxide of cerium, and the filtered liquor contained oxide of iron tolerably pure. Muriate precipitated by oxalate of ammonia.

Exp. 5. The liquor freed from the greater part of the iron by ammonia and heat, which had notwithstanding a slight roseate tinge, was precipitated by oxalate of ammonia. The precipitate at the moment of its formation had the appearance of muriate of silver, but soon became granulous and subsided in this form. The liquor passed through the filter colourless, and the rosy tinge remained in the oxalate. Nitrate precipitated by oxalate of ammonia.

Exp. 6. As the liquor from which the oxalate of cerium was precipitated contained an excess of acid, it might be presumed to hold in solution most of the oxalate of lime formed at the same time, if the cerite contained any. Accordingly it was mixed with the water that had washed the precipitate and concentrated by evaporation, when on the addition of ammonia a small quantity of oxalate of lime was thrown down. Oxalate of lime separated.

Exp. 7. As notwithstanding some oxalate of lime might have been precipitated with the oxalate of cerium, a portion of the Rest of the lime separated.

red

red oxide of cerium arising from the decomposition of the oxalate by calcination was dissolved in muriatic acid. A brisk effervescence instantly took place, with the evolution of oxigenated muriatic gas, which continued till the whole was dissolved, and differed in no respect from that prepared with oxide of manganese.

Muriatic solution of cerium rendered solid by ammonia.

The solution of cerium in muriatic acid was clear, and had only a light rosy tinge. To separate it from the lime, if there were any, ammonia was added, when the solution, having been diluted with but a small quantity of water, congealed into a semitransparent gelatinous mass, which it was necessary to agitate with a great deal of water, before it could be gotten out of the bottle.

The precipitate being washed and calcined was very compact, and had a brilliant fracture.

The liquor thus decomposed by ammonia contained lime, as appeared on precipitating it with oxalate of ammonia.

Oxalate of cerium.

At the instant when the oxalate of cerium is precipitated by ammonia it is white and semitransparent; but by agitation in the air and desiccation it assumes a yellowish colour, and becomes opake. A remarkable circumstance is, that, if it be boiled with ammonia or potash before it is dry, it becomes again perfectly white and opake. This is not owing to any combination of the alcalies with the cerium, for when it has been well washed, no trace of them can be discovered by the most careful analysis.

Does not combine with alkalies.

Component parts of cerite.

The residuum left untouched by the acids was afterwards examined; when it appeared, that the purest ore of cerium * from Bastnas contained in 100 parts,

Oxide of cerium		63
Silex	- -	17·5
Oxide of iron	-	2
Lime	- -	3 or 4
Water	-	12
		98·5 †

Cerium,

* Mr. Vauquelin analysed other specimens, which were mixed with green actinote and cupreous pyrites; but as nothing particular occurred in these analyses, it is unnecessary to enter into them.

† Messrs. H. and B. say: silex 23 parts, carbonate of lime 5·5, oxide of iron 22, and of oxide of cerium after calcination more than 50.

Cerium, like several other metals, appears susceptible of two very distinct degrees of oxigenation: the oxide which contains least oxigen is white; that which is saturated with it is of a fallow red. Though they differ considerably in certain respects, their quantities of oxigen are not very dissimilar, whence they are readily and easily commutable into each other. Cerium has two oxides.

The white oxide exposed to the blowpipe soon becomes red, but does not melt, or even agglutinate. With a large proportion of borax it melts into a transparent yellow globule *: with less the globule becomes opake on cooling. On heating gently a transparent compound of borax and oxide of cerium it becomes milky like a tin enamel. Exposed to the blowpipe with borax.

The white oxide of cerium becomes yellowish in the open air, but never so red as by calcination, because it readily combines with carbonic acid, which opposes its union with oxigen to the point of saturation, and because it always retains a portion of water, which diminishes its colour. Takes oxigen and carbonic acid from the air.

Caustic potash by the assistance of heat deprives the red oxide of part of its oxigen, and renders it white. This being dried, however, and urged to the state of fusion, becomes red again. Alcalies have no other action on it. Alcalies do not act on it.

Sulphuric acid dissolves the red oxide with great difficulty. Equal parts of it and of sulphuric acid diluted with four times its weight of water combine readily when heated: the whole mass assuming a crystalline form and brilliant aspect. On adding fresh acid, and heating them together a long time, a complete solution takes place. This solution being evaporated by a gentle heat crystallizes in very small needles, some of which are orange †, others of a lemon colour. If evaporated quickly, nothing but a yellow powder is obtained. Sulphuric acid with the red oxide. Two sulphates.

50. The increase of weight they ascribe to oxigen absorbed by the iron and the cerium.

* First blood-red, then, as the heat decreases, green, yellowish, and finally colourless. If it be kept in the middle of the flame it continues as clear and colourless as glass. These phenomena are more evident, if a phosphoric salt be employed. If two colourless transparent globules, one formed with borax the other with a phosphoric salt, be fused together, a transparent compound is produced, which on cooling becomes opake, and of a pearl colour. Messrs. H. and B. With a phosphoric salt.

† These Messrs. H. and B. consider as an acidulous sulphate of cerium at a maximum of oxidation.

The

The sulphate of cerium is soluble in water only by means of an excess of acid. Its taste is saccharine and acid.

With the white oxide. Sulphuric acid easily combines with the white oxide, particularly in the state of carbonate. The solution is colourless, or with a slight rosy tinge; of a saccharine taste without any perceptible acidity; and readily affords white crystals.

Nitric acid with the red oxide. Nitric acid does not readily dissolve the red oxide unless assisted by heat. If the acid be superabundant, the solution yields white deliquescent crystals: if not, no crystals are formed, but a yellowish salt is formed by desiccation, of which alcohol at 38° will dissolve half its weight. The nitrate of cerium is decomposable by heat, and leaves a brick-coloured oxide.

With the white. The white oxide unites more readily with nitric acid, but this salt is not more easily crystallizable. Its taste is at first pungent, afterward very sugary.

Muriatic acid. Muriatic acid dissolves the red oxide with effervescence. The solution crystallizes confusedly. The salt is deliquescent, soluble in an equal weight of cold water, and in three or four times its weight in alcohol. The flame of this solution acquires no colour from the salt, but if agitated, white, red, and purple points appear in it *.

Oxigenated muriatic acid. Oxigenated muriatic acid has no action on the red oxide, but dissolves the white, without yielding to it any of its oxigen.

Carbonic acid. The oxide of cerium unites easily with carbonic acid. The most simple and ready method of forming this compound is to decompose a solution of the nitrate or muriate of the white oxide by saturated carbonate of potash, when a very white precipitate will be formed with effervescence, which is very light, and on drying assumes a shining silvery appearance.

Hidro-sulphures separate iron from cerium. Sulphurated hidrogen does not combine with cerium: but hidrosulphures may be employed successively to separate any iron that may be mixed with it; for, when this is the case, the first portions of hidrosulphure will throw down from the solution of cerium a greenish precipitate till no more iron remains.

Tartarous acid. The white oxide will unite directly with tartarous acid, but requires an excess of the acid to render it soluble †.

Mr.

* When this solution is concentrated it burns with a yellow sparkling flame. Messrs. H. and B.

† Messrs. H. and B. have observed, as well as Mr. V. that, if the

Mr. Vauquelin made several unsuccessful attempts to reduce this metal; at first he used the oxalate made into a paste with fat oil. However, having mixed tartrite of cerium with a very small quantity of oil and lamp-black, he put it into a crucible of charcoal bedded in sand in an earthen crucible, and heated it for an hour and half in a forge furnace. A metallic globule scarcely as large as a pin's head was now left in the coal, but no other trace of cerium could he discovered, though the sand was examined with the utmost care. Reduction of the metal.

None of the simple acids acted on this globule, but it dissolved, though with extreme difficulty, in aqua regia, after being triturated. The solution was reddish, and exhibited unequivocal marks of iron: but it likewise gave evident signs of the existence of cerium, both by its saccharine taste, and by the white precipitates which tartrite of potash and oxalate of ammonia threw down. The metallic globule too was harder, much more fragile, more scaly in its fracture, and more white than pure cast iron. The globule examined.

As from these experiments cerium appears to be volatile, a similar mixture with the addition of borax was heated in a porcelain retort, to the neck of which a porcelain tube was adapted. Whether from the insufficiency of the heat however, or from the metal being volatilized without adhering to the neck of the retort, no trace of sublimate was found. But several very small metallic globules remained in the retort, adhering to a black varnish formed by the borax. There were some of these globules about the upper part of the vessel, to which apparently they had been sublimed by the force of the fire; but all these globules together would not have amounted to a fiftieth part of the cerium employed. Volatile, but attempt to sublime it fruitless.

the salts of cerium, decomposed by tartrite of potash still contain traces of iron, the iron remains dissolved in the liquor, particularly if a slight excess of tartrite be employed. Accordingly they have proposed this method as the best and simplest for freeing the cerium from iron. The process they recommend for obtaining pure oxide of cerium is, to dissolve in nitro-muriatic acid any quantity of cerite, carefully selected and thoroughly calcined. To filter the solution, neutralize it by caustic potash, and then precipitate by tartrite and potash. The precipitate well washed, and afterward calcined, is pure oxide of cerium. Simple and ready method of freeing cerium from iron, and obtaining the oxide pure.

Abstract

IX.

Abstract of a Memoir, entitled Considerations on Colours, and several of their singular Appearances; read at the Class of Mathematical and Physical Sciences of the National Institute, March, 1805, *by* C. A. Prieur *.

Object of the memoir.

OUR author here endeavours to account for several phenomena, which appear to him never yet to have been properly explained: or rather it is his object to exhibit a general theory, by means of which all cases of coloured appearances, even the most extraordinary, may be referred to certain principles.

Begins with the colours resulting from a mixture of rays.

He sets out from the known opinions concerning the various species of luminous rays, the colours resulting from a mixture of several of these rays taken at different parts of the solar spectrum, and among others the very remarkable case, where the rays are so chosen, that their union produces on the organ of sight the sensation of whiteness, even if two sorts of rays only be employed.

For which we are indebted to Newton.

For these ideas we are indebted to the discoveries of the immortal Newton, and they flow immediately from the method he has proposed for determining what colour would be obtained from a mixture of certain quantities of other given colours.

Preliminary requisites.

If we would thoroughly comprehend what passes in the seeing of colours, it is indispensable in the first place to obtain a familiar acquaintance with the shades composed of several simple rays; to acquire precise ideas of *black* and of *white*, and of the complication these introduce into coloured appearances; and more especially to understand the relation of colours, which, taken two and two in a certain order, are capable of forming by their union white, or if you please any other compound tint.

Complementary colours.

Two colours having this kind of relation to each other are reciprocally termed *complementary colours*: one of these being given, the other may be determined with more or less precision by various modes of experiment, calculation, or simple reasoning; and the consideration of them applies very usefully to a great number of cases, as will be seen farther on.

* Translated from the *Annales de Chimie*, Vol. LIV. p. 5, April, 1805.

We

We here pass over many particulars, which persons versed in the science of optics, or habituated to the practical application of colours, will easily supply. Besides, the subsequent part of the memoir, of which we have undertaken to give an account, will furnish an opportunity of repeating what is most necessary for understanding these subjects.

Contrasts.

After these preliminaries the author proceeds to observations on *contrasts*. He employs this word to characterize the effect of the simultaneous vision of two substances differently coloured, when brought near together under certain circumstances. Contrast then is here a comparison, from which results the sentiment of a certain difference, great or small. It is pretty generally known, and painters in particular are well aware, that a coloured substance occupying a space of little extent, and placed near or surrounded by a given colour, has not the same appearance as in the neighbourhood of another colour: but whence arises this difference?

Before we attempt to answer this question, let us make an essential distinction. The colours in question must be either homogeneal, that is formed of one sort of rays only; or compound, that is formed of a mixture of different rays.

Contrasts of simple colours not yet examined.

In the first case, it must be confessed, we are ignorant, whether the approximation of different simple colours would produce any alteration in their respective appearance. As we seldom have an opportunity of seeing exhibitions of colour of this kind, and it is not easy to arrange such at will, no experiments have yet been made on their contrasts. The subject, however, is well worth studying.

Its effect produced by abstracting from a colour the rays analogous to that contrasted with it.

As to compound colours, and such are almost all those of natural or artificial substances, as our author shews in the course of his paper, the new colours exhibited by contrast are always conformable to the tint that would be obtained by abstracting from the colour proper to one of the substances the rays analogous to the colour of the other.

Orange on red appears yellow; on yellow, red; on violet, yellowish; on green, another red:

Thus if we place on red paper a slip painted orange-colour, the latter will appear nearly yellow: on the contrary, the same strip placed on yellow paper will appear nearly red. If we place it on violet paper it will resume a yellowish tint, but different from the former; and lastly, on green paper it will appear red, but in a different degree.

because orange consists of all rays but blue.

The explanation of these instances by the rule proposed is easy, if we suppose the orange-colour of the little strip to be compounded of all the rays except blue, which is commonly the case.

Contrast modified by circumstances.

A multitude of combinations of colours thus placed upon one another, bring out the colour of contrast indicated by the rule above laid down; but there are several circumstances that render the effect more striking, or modify the result.

Degree of light.

Many contrasts at once.

Sometimes it depends on the degree of light by which the colours are observed. They may be illumined uniformly, or some more than others. The quantity of light entering simultaneously into the eye from the whole field of view, has likewise its influence. If the colours form several surrounding borders to each other, as a series of circles decreasing in size and placed one upon another would do, they will act reciprocally on each other. At every junction there will be on each side a border coloured by the contrast of the adjacent tint. These borders will be of greater or less extent in proportion to the brightness of the colour. The effect of a single one may be sufficient to deaden or annihilate the rest.

Effect increased by slight fatigue of the eye.

But not by excessive.

The colours of contrast will appear likewise with greater vividness after having observed them a few moments, or if the coloured substances be shaken a little, so that they may pass slowly over the retina. It seems as if a certain fatigue of the eye, either instantaneously with regard to the intensity of the light, or more slowly by a prolonged vision, concurred to produce the appearances in question. But an excessive fatigue of the organ would produce a degeneration of the colours belonging to another mode.

Colour on the retina after exposure to strong light, not from contrast.

We ought not therefore to refer to contrast those impressions mentioned by Æpinus, which are propagated in the eye with a certain duration, and a particular period of tints, when we have looked stedfastly on a very brilliant light, as that of the sun.

Buffon's accidental colours are

But the colours termed by Buffon *accidental*, on which Scherfer has written an interesting essay, belong to the class of contrasts, or at least constantly observe the same law.

coloured shadows of the same nature;

Coloured shadows are another phenomenon of the same kind. Count Rumford has established this fact beyond question in two

wo essays, where he has treated the subject in a very pleasing manner.*

Mr. Prieur thinks, that those appearances of the solar light received through a hole in a coloured curtain, which General Meusnier had remarked on account of their singularity, are also to be ascribed to contrast. With this too he assimilates several cases of colours displayed by opals, or, to speak more generally, by bodies including perceptible opake parts disseminated through a transparent substance. In the same way he explains the colours under which the grayish dust collected by age on papers, or on coloured stuffs, appears; and he draws the same inferences with respect to the blueish appearance of the veins of the human body.

also light through hole in coloured curtain;

opals;

old dust on paper and cloth,

and blueness of the veins.

He likewise proposes a new method of rendering the colours of contrast very sensible, more so than even by the known process of accidental colours, and nevertheless without occasioning any extraordinary fatigue of the eye. This last circumstance is of no small consequence, for every one must be aware, that so delicate an organ cannot be strained by over exertion without danger.

This method consists, the observer being in a room with a good light, in placing against the window the coloured papers, on which he means to observe the contrasts in the manner above mentioned. The coloured paper serving as the ground will then possess a degree of semitransparency, while the little slip of a different colour placed upon it is more opake, and in the shade, on account of the double thickness of paper: thus the colour produced by the contrast is rendered much more striking.

Method of rendering contrasts very sensible.

From this arrangement too results the singularly striking effect of contact of a little slip of white paper applied successively on paper, glass, and cloth of a given colour. When the transparent body is red, the opake white appears blueish green; if the ground be orange, it is decidedly blue; on a yellow ground, a kind of violet; on a crimson ground, green, &c.; always corresponding exactly to the *complementary colour*.

Slip of white paper on coloured paper, glass, &c.

On this it must be observed, that, according to the rule already mentioned, if we abstract from white, which is a com-

Explanation.

* See his Philosophical Works, Vol. I. p. 319, and following.

pound of all the coloured rays, the red rays for example, the remaining pencil ought to appear a very pale blueiſh green: but, as in the experiment above the little white ſlip is in the ſhade, the black hence ariſing may be of a proper degree to deſtroy the effect of the white, and then the blueiſh green appears of a lively tint. The ſame reaſoning is applicable to the caſe of all the other colours.

Reflected light muſt be avoided.

To obtain the full effect in repeating theſe experiments, we muſt take care, while procuring a favourable light, to guard againſt the reflection of adjacent bodies, and againſt double coloured fringes. Thus when the bright light tranſmitted through the window ſurrounds the tranſparent paper, it may very ſenſibly augment the brightneſs of the colour of contraſt, or injure it by introducing another tint, according to the colour of the body under obſervation. We have it always in our power, however, to get rid of this ſupercompoſition, by taking a piece of black cloth or paſteboard to maſk the object thus incommoded, or by looking through a blackened tube ſo as to confine the field of viſion to the neceſſary extent.

How.

Uſeful in the arts.

This knowledge of contraſt may be uſefully applied to thoſe arts, which are employed on the ſubject of colours. The painter is aware, that it is not a matter of indifference what colour is placed near another: but when he is acquainted with the law, to which their action on each other is ſubjected, he will know better what to avoid, and how to diſpoſe his tints, ſo as to heighten the brilliancy of that which he wiſhes to bring forward. Contraſting them together in ſucceſſion likewiſe affords us valuable indications of their nature and compoſition. This the author himſelf has put in practice with advantage in his manufactory of colours and paper-hangings.

White appearance of a coloured body through glaſs of the ſame hue.

Theſe conſiderations on contraſts led him to the examination of a very ſingular caſe, which Mr. Monge has mentioned and treated with his uſual ſagacity *. This caſe is the white appearance, which a coloured body ſometimes exhibits when viewed through a glaſs of the ſame hue. There remained ſome uncertainty reſpecting the circumſtances actually neceſſary for producing this effect: theſe our author determines by particular experiments, and he enumerates thoſe which have a favourable influence or the contrary. His concluſion is, that,

* *Annales de Chimie*, Vol. III.

when

when we have the perception of whiteness in these cases, it is owing solely to the action of contrasts, by which the impression of the colour is deadened or annihilated; while that of a certain degree of brightness still subsists, and is noticed from the opposition of a greater degree of obscurity. This manner of considering the subject leads to a new definition of whiteness which has certainly nothing in it inconsistent: *white is with respect to us the sensation of light, when no particular colour predominates in it, or is perceived in it.*

New definition of whiteness.

In the subsequent part of his memoir our author particularly considers the colouring of different opake and transparent bodies; that is to say, he inquires what are the luminous rays which a given coloured body is really capable of reflecting or transmitting.

Further subject of inquiry.

His method of making his experiments is simple. If the substance be opake, he places it on a piece of black cloth, and observes it with the prism. If it cannot be cut so as to reduce it to a rectangular figure, he covers it with a piece of blackened pasteboard, in which there is a rectangular aperture. Under these circumstances the coloured fringes displayed on two opposite sides indicate the kind of rays reflected, and consequently those absorbed when we know the nature of the illuminating pencil. On which we have farther to remark, that, as the fringes are themselves compound tints, the simple tints that compose them must be discriminated. Their inspection suffices an experienced person for this; but the habit is to be acquired, and its place supplied, by taking for a guide papers representing each species of rays, placing them in their order one upon another, and drawing them back in gradation conformably to their difference of refrangibility: or we may use a table constructed after Newton's method for determining the compound tints of several elementary colours.

Method of making his experiments.

Compound tints to be discriminated.

How.

If the body to be examined be transparent, the aperture in the pasteboard just mentioned will be well adapted to cover it when placed against the light, so that the prism may exhibit fringes on it. Or, if the observer place himself in the dark, a light, as that of a candle, will exhibit through the transparent substance, by the assistance of the prism, a series of coloured images corresponding to the rays transmitted.

Method of examining transparent bodies.

Making his experiments in this manner, our author discovered that several opake substances which happened to be at hand,

Colours of opake bodies owing to absorption.

hand, of various natures and of all colours, whether yellow, orange, red, or green, blue and violet, owed their coloured

Laws.

appearance to the following laws: 1ſt, each of the bodies always abſorbed the rays that were complementary to the predominant colour: 2dly, in ſome the abſorption included, beſide the complementary ſpecies, others collateral to this ſpecies, and more or leſs numerous: 3dly, the deeper a colour is, the fewer ſpecies of rays it reflects.

Relates to chemical compounds, not mechanical mixtures.

It is to be underſtood that mixed colours are not here ſpoken of, but only thoſe that form a homogeneal compound, or a true *combination*, in the ſenſe in which chemiſts uſe this word. Nor muſt the colour reflected from the interior of the molecules, ſuſceptible of light or deep tints, be confounded with the light reflected from the anterior ſurface of bodies: and though this mixes more or leſs with the proper colour, it is eaſy to diminiſh its effects, and diſcriminate them in the experiments.

Predominant colour.

Another remark proper to be made, is, that the expreſſion *predominant colour* muſt not be ſuppoſed to imply, that the rays of this colour are more abundant than the reſt, which would be a miſtake. Several ſpecies of rays may exiſt together in the pencil producing the colour, without any one ſpecies being for this reaſon more abundant. Strictly ſpeaking, all the elements of the pencil are diſſimilar; and conſequently no one exiſts in it in greater quantity. But the general tone of colour remains analogous to that of the rays ſtyled predominant; for which reaſon it is well to retain the term, provided it be not taken in an exaggerated ſenſe.

Tranſparent bodies follow the ſame law of ablo pti

The author has likewiſe obſerved tranſparent bodies, ſuch as coloured glaſs of different ſorts, and liquors contained in a bottle with two broad parallel ſides. For theſe he found the ſame law of abſorption as for opake bodies, but ſtill more marked, and free from all doubt.

Its modifications.

This law is conſtant and regular. It depends on the nature of the body receiving the light, its denſity, and its thickneſs. It is likewiſe modified by the intenſity of the light of the illuminating body, and the kind of rays that compoſe this light.

Progreſs of the abſorption of rays.

The abſorption always commences with the rays moſt oppoſite to the predominant colour of the body illumined. It goes on to thoſe which come next in the ſpectrum; and thus proceeds regularly from one order of rays to the next in ſucceſſion,

ceſſion,

ceſſion, never by fits, till it reaches the laſt. In conſequence the body grows darker and darker, and always finiſhes with becoming black. Sometimes it extends only on one ſide from the rays firſt abſorbed; at other times on both ſides at once, and either with equal pace, or more rapidly on one ſide than on the other.

If we vary each particular that affects the experiment ſeparately, we ſhall have a diſtinct progreſſion of reſults. That depending on the denſity of the ſubſtance is not always ſimilar to that ariſing from change of thickneſs. In receiving light of different kinds too on the ſame ſubſtance, the progreſs of abſorption is differently modified, and conſequently the colours changed.

Change of circumſtances varies the reſults.

Our author adduces inſtances of all theſe caſes. He takes them from the numerous experiments he has made with coloured glaſs, acid, or alcaline ſolutions of metals, and fluids tinged by the infuſion or decoction of vegetable ſubſtances. Theſe exhibit curious particularities, but we ſhall not here relate them, both for the ſake of brevity, and becauſe it is eaſy for any perſon to obſerve them, when once the track is pointed out.

From all theſe obſervations taken together, many very important conſequences reſpecting the reciprocal action of bodies and light on each other are drawn; and perhaps at ſome future period they will tend to elucidate the grand queſtion concerning the cauſe to which their permanent colours are to be aſcribed.

May lead to the cauſe of permanent colours.

After theſe hints, the author dedicates a concluding paragraph to the examination of ſeveral phenomena of different kinds. He points out the modifications that coals heated to different degrees of incandeſcence undergo in their colours. His remarks apply to other ſubſtances likewiſe, as iron in the ſtate of ignition, a long row of lamps with reverberators ſeen through a fog, or a white light ſeen through a glaſs blackened by progreſſive applications of ſmoke. In all theſe caſes the colours neceſſarily paſs through a ſeries of tints from white to yellow, orange, and red of a deeper and deeper ſhade, the reaſon of which he gives.

Colour of bodies at different degrees of incandeſcence.

Lamps ſeen through miſt.

Sun through ſmoaked glaſs.

Metallic oxides too have a gradation of tints, according to their proportion of oxigen. A certain continued change in vegetation

Colours of metallic oxides proportionate to the oxigen.

Of flowers, &c. vegetation produces the same effect on some parts of plants. The arts and chemical processes exhibit the same in a multitude of circumstances.

Use to the manufacturer. Hence the manufacturer may derive with advantage indications either of the progress of combinations, or of the proper instant for executing certain parts of his operations.

Coloured clouds. Our author next enters more particularly into the appearance of coloured clouds, particularly those we see about the rising and setting of the sun. This phenomenon so generally known, had hitherto remained without explanation, though this had been attempted by natural philosophers of the first rank.

Owing to absorption of light, not to refraction. It is not owing to the refraction of the solar rays, but to the successive absorption of them, when they strike on the inferior parts of the atmosphere, which are more loaded with vapour.

This absorption follows laws analogous to those already mentioned. The quantity of vapours, and even their nature not being the same every day, produce corresponding differences in their effects.

Order of absorption. Commonly the first rays attacked by these vapours are the blue adjacent to the violet. Soon after they attack the contiguous rays, gaining with more rapidity the blue properly so called; then the green, the yellow, and thus proceeding to the red. Hence the yellowish, orange, and red colours exhibited by the clouds. This period of tints, the evening for example, displays itself gradually as the sun approaches the horizon. The same hues tinge terrestrial objects, the part of the air nearest the sun, and this luminary itself. Accordingly when we can receive its rays on a prism, we perceive that the rays actually absorbed correspond to the general tint of the moment.

Sun-set.

From the successive increase of the vapours traversed by the light in thickness and density, it follows likewise, that at the same instant clouds differently placed must be clothed in different hues. The highest may be white, while others not so high are yellow, and others still lower proportionately more red. At equal elevations those furthest from the point where the sun sets will incline to red, and those nearest it to yellow.

Blue and green shadows, owing to contrast. We may then see blue or green shadows on bodies naturally white, as Buffon and other philosophers have observed. These, as has been said, are nothing more than the effect of contrast between

between the actual colour of the part enlightened, and that of the part in shade.

Contrasts may likewise render the colour of the clouds complicated, as for instance, when a great portion of the sky displays its blue tint. There are some clouds, the colour of which arises solely from this cause; and such may be seen at times in the middle of the day, when we have a lofty mountain at our back, or are in any other situation where the eye is defended from the too powerful action of the solar light, either direct or reflected; but in this case the clouds have only a yellowish tinge, precisely the complementary colour of sky blue. Contrasts affect the colour of the clouds.

Sometimes we see the moon of a similar colour, when it is very high, a little before or after the sun passes the the horizon: farther it appears thus, or even completely white, when clouds variously coloured by the vapors of sunset or sun-rise exist in the air at the same time. From this concurrence of circumstances we have a new proof of the difference of causes to which these colours are owing. The Moon.

Lastly let us remark, that from the irregularity of the earth's surface, and of the state of the atmosphere, the phenomena are liable to be concealed or subjected to various interruptions. In our climate the colouring of the clouds seldom reaches its last stage. On some evenings however, when the sky is very clear toward the part where the Sun sets, while light clouds float very high over our heads, we shall see these at a subsequent period appearing of a very light red, heightened by the diminution of light on the earth, soon after obscured, and at length becoming extinct in shade. Red clouds over head at sun-set.

Conclusion.

Notwithstanding the many beautiful discoveries already made respecting light, the theory of the production of colours has not yet attained a degree of generalization that renders it applicable to all cases, or that simplicity of principles to which we are almost always led when we have discovered the real laws of nature. Many phenomena have eluded explanation, and that given of several requires correction. Our author has proposed to establish alterations in the theory, the necessity of which he points out. He supports his principles partly by the doctrine and facts generally admitted; partly by The theory of colours imperfect.

by others less commonly known, though of ancient date; and lastly by observations of his own. He is far from flattering himself, however, that a sketch like the present exhibits the matter in a suitable light; and was soon aware, that a subject so extensive and so complicated required maturer labours.

The author intends to pursue the subject.

To fill up many gaps, unfold various points, and correct and extend others by farther researches, new experiments, and profound reflections, is an ample field of improvement; and this he will attempt, if his powers and his leisure will permit.

It would likewise be useful, as well as just, to give at the same time an abstract of what we owe to the genius of the great Newton, who opened the career in such an admirable manner, and to those philosophers who have discovered new facts, or removed difficulties. Greater precision also should be introduced into the language which we employ respecting colours, proportionate to the increase of our knowledge, and the actual state of the arts and sciences. Lastly, in a subject like the present, it would not be too much to add the resources of algebra and geometry to the treasures of experiment, and if possible to the advantages of a better method.

X.

Report made by the Physical and Mathematical Class of the Institute in Answer to the Question, whether those Manufactories, from which a disagreeable Smell arises, may prove injurious to Health. Read in the Sitting of January, 1805, by Messrs. GUYTON-MORVEAU and CHAPTAL.*

THE minister of the home-department has consulted the class on a question, the solution of which is of essential import to our manufacturers.

Question.

The object is to determine, whether the vicinity of certain manufactories can be injurious to health.

Its importance.

The solution of this problem must appear of the more consequence, as, from the confidence which the decisions of the Institute naturally merit, it may hereafter form the basis of

* Translated from the *Annales de Chimie*, vol. LIV. p. 86, for April, 1805.

decisions

decisions in a court of justice, when sentence is to be pronounced between the fate of a manufactory and the health of our fellow-citizens.

The solution is so much the more important, it is become so much the more necessary, as the fate of the most useful establishments, I will say more, the existence of many arts, has depended hitherto on simple regulations of police; and that some, driven to a distance from materials, from workmen, or from consumers, by prejudice, ignorance, or jealousy, continue to maintain a disadvantageous struggle against innumerable obstacles, by which their growth is opposed.

Thus we have seen manufactories of acids, of sal ammoniac, of Prussian blue, of beer, and of leather, successively banished from cities; and we daily see appeals to authority against these establishments made by troublesome neighbours or jealous rivals. Manufactories objected to.

As long as the fate of these manufactories is insecure, as long as an arbitrary legislation possesses a right to interrupt, suspend, or fetter the hands of a manufacture; in a word, as long as a simple magistrate of police has at his nod the fortune or ruin of a manufacturer, how can we conceive, that he will be so imprudent as to engage in undertakings of such a nature? How could it be expected, that manufacturing industry should establish itself on such a frail basis? This state of uncertainty, this continual contest between the manufacturer and his neighbours, this perpetual doubt respecting the fate of an establishment, paralyse and confine the efforts of the manufacturer, and gradually extinguish both his courage and his powers. Disadvantage of having no fixed rules.

It is an object of primary necessity therefore to the prosperity of the arts, that lines should be drawn, so as no longer to leave any thing at the arbitrary will of the magistrate; to point out to the manufacturer the circle in which he may exert his industry with freedom and security, and to assure the neighbouring proprietor, that he has nothing to fear for his health, or for the produce of his fields.

To arrive at the solution of this important problem, it appeared to us indispensable, that we should take a view of each of the arts, against which the most clamour has been raised.

With this view we shall divide them into two classes. The first will comprise all those, the processes of which allow aeriform emanations to escape from them into the atmosphere, either Classification of objectionable trades.

in

in consequence of putrefaction or fermentation, which may be deemed nuisances from their smell, or dangerous from their effects.

The second class will include all those, in which the artist, operating by the aid of fire, developes and evolves in air or vapour various principles, which are more or less disagreeable to respire, and reputed more or less injurious to health.

1st class. In the first class we may advert to the steeping of flax and hemp, the making of catgut, slaughter-houses, starch-manufactories, tanneries, breweries, &c.

2d class. In the second, the distillation of acids, of spirits, and of animal substances; gilding on metals, preparations of lead, copper, and mercury, &c.

1st class injurious to health. The arts of the first class, considered in relation to the health of the public, merit particular attention, because the emanations that proceed from fermentation or putrefaction are really injurious to health in some cases, and under certain circumstances: the steeping of flax and hemp for instance, which is performed in ponds or still waters, infects the air and kills fishes; and the diseases to which it gives rise are all known and described: Accordingly wise regulations have almost every where enjoined, that this operation should be carried on without the precincts of towns, at a certain distance from every dwelling, and in waters, the fish of which constitute no resource for the public. These regulations unquestionably ought to be continued; but as the execution of them is attended with some inconvenience, it is to be wished, that the process of Mr. Brale, the superiority of which has been confirmed by Messrs. Mongez, Berthollet, Tessier, and Molard, should soon become known and adopted.

Steeping hemp.

Brale's method recommended.

Beer, vegetable colours, starch, paper, &c. Other operations on vegetables, or certain products of vegetation, to obtain fermented liquors, as in breweries; to extract colours, as in the manufactures of litmus, archil, and indigo; or to divest them of some of their principles, as in manufactories of starch, paper, &c. do not appear to us of such a nature as to be capable of exciting any disquietude in the mind of the magistrate. At all events the emanations arising from these substances in a state of fermentation can prove dangerous only near the vessels and apparatus in which they are confined, ceasing to be so the moment they are mingled with the open air; so that a little prudence only is required, to avoid all danger.

danger from them. Besides, the danger affects only the manufacturers themselves, and by no means the inhabitants of the neighbouring houses, so that a regulation enjoining these manufactories to be removed out of towns, and to a distance from any dwelling-house, would be an act of authority both unjust, vexatious, and injurious to the progress of manufactures, and in no respect a remedy for the evils attending the operation.

Some preparations extracted from animal substances require the putrefaction of these substances, as in the fabrication of catgut; but it is more frequently the case, that animal substances employed in manufactures are liable to putrefaction from being kept too long, or exposed to too great warmth, as we particularly find in dyeing cotton red, a process in which a large quantity of blood is employed. The miasmata exhaled by these putrid matters spread far round, and form a very disagreeable atmosphere for all the neighbourhood to breathe; it is the part of a good government, therefore, to cause these substances to be renewed so as to prevent putrefaction, and the manufactory to be kept so far clean, that no refuse of the animal substances employed shall be left to rot in them.

Catgut.

Dyeing cotton red.

In this last point of view slaughter-houses exhibit some inconveniencies; but they are not of sufficient importance to require them to be placed without the precincts of towns, and assembled together in one spot, as speculative men are daily proposing to government. A little attention on the part of the magistrate, to prevent butchers from throwing out the blood and refuse of the beasts they kill, would be sufficient to remedy completely every thing disgusting or unhealthy arising from slaughter-houses.

Slaughter-houses.

The fabrication of *poudrette* (night-soil dried) begins to be established in all the large towns of France, and the operation by which excrementitious substances are reduced to this state, necessarily occasions a very disagreeable smell for a long time. Establishments of this kind therefore ought to be confined to airy places, remote from any habitation; not that we consider the aeriform exhalations from them as injurious to health; but no one can deny, that they are incommoding, noisome, disagreeable, and difficult to breathe, on all which accounts they ought to be removed to a distance from the dwellings of men.

Poudrette.

There is a very important observation to be made on the spontaneous decomposition of animal substances, which is, that

Animal putrefaction dangerous only in proportion to its humidity.

that the emanations from them appear to be so much the less dangerous, as the substances which undergo putrefaction are less humid: in the latter case, a considerable quantity of carbonate of ammonia is evolved, which imparts its predominant character to the other matters volatilised, and corrects the bad effects of such as are deleterious. Thus the decomposition of stercoraceous matters in the open air, and in places the situation and declivity of which allow the fluids to drain off, and that of the refuse of the cocoons of the silk-worm evolve a vast quantity of carbonate of ammonia, which corrects the virus of some other emanations; while the very same substances, decomposed in water or drenched with this fluid, exhale sweetish and nauseous miasmata, the respiration of which is very dangerous.

2d class. The numerous arts in which the manufacturer produces and diffuses in the air, in consequence of his processes and by the help of fire, vapours more or less disagreeable to breathe, constitute the second class of those we have to examine.

These, more interesting than the former, and much more intimately connected with the prosperity of our national industry, are still oftener the subject of complaints brought before the magistrate for decision, and on this account have appeared to us to require more particular attention.

We will begin our examination with the manufacture of acids.

Acids. The acids that may excite complaints of the neighbours against their preparation are the sulphuric, nitric, muriatic and acetous.

Sulphuric acid. The sulphuric acid is obtained by the combustion of a mixture of sulphur and nitre. It is very difficult in this process to prevent a more or less observable smell of sulphurous acid from being diffused around the apparatus, in which the combustion is performed; but in manufactories skilfully conducted this smell is scarcely perceptible within the building itself, is not dangerous to the workmen who respire it daily, and can give no reasonable foundation for complaint to the neighbours. When the art of making sulphuric acid was introduced into France, the public opinion was strongly expressed against the first establishments for the purpose; the smell of the match with which we kindle our fires contributed not a little to exaggerate the effect that must be produced by the rapid com-

bustion of several hundred weight of brimstone; but men's fears on this head are now so much allayed, that we see several of these manufactories prosper in peace in the midst of our cities.

The distillation of aqua fortis and spirit of salt, in other words, of the nitric and muriatic acids, are not more dangerous than that of sulphuric acid. The whole of the process is performed in an apparatus of glass or earthen-ware, and it is unquestionably the great interest of the manufacturer to diminish the volatilization or loss of the acid as much as possible. Yet, let him pay whatever attention he will to this, the air breathed in the manufactory is always impregnated with the smell peculiar to each of these acids; but you may respire there freely and safely, the men who work in it daily are not at all incommoded by it, and the neighbours would be very much in the wrong to complain. Aqua fortis and spirit of salt.

Since the manufactories of white lead, of verdigris, and of sugar of lead have increased in France, the demand for vinegar has been enlarged. Vinegar.

When this acid is distilled, to fit it for some of the purposes for which it is used, it diffuses to a distance a very strong smell of vinegar, in which there is no danger; but when a solution of lead in this acid is evaporated, the vapours assume a sweetish character, and produce in those who respire them constantly all the effects peculiar to the emanations of lead itself. Happily these effects are confined to the people who work in the manufactory, and are unfelt by those who dwell in the vicinity. Distilled vinegar.

The preparations of mercury and of lead, those of copper, antimony, and arsenic, and the processes of gilding on metals, are none of them without some danger to the persons who reside in those manufactories, and are concerned in the operations; but their effects are bounded by the walls within which they are carried on, and are dangerous only to the persons concerned in the manufactories. It is an object well worthy the attention of chemists, to investigate the means of preventing these injurious effects, and indeed many of the inconveniencies have already been prevented by the help of chimneys, which convey the vapours into the air out of the reach of respiration: and at present the whole attention of administration ought Mineral preparations, and gilding.

ought to be confined to directing science toward the means of improvement of which these processes are susceptible with regard to health.

Prussian blue, and sal ammoniac.

The fabrication of Prussian blue, and the extraction of carbonate of ammonia by the distillation of animal substances in the new manufactories of sal ammoniac, produce a large quantity of fetid vapours or exhalations. These exhalations, it is true, are not injurious to health; but as it is not sufficient to constitute a good neighbour, not to be a dangerous one merely, but not even to be a disagreeable one, they who undertake such manufactures, when they have to seek a situation for them, should prefer one remote from any dwelling-house. But when such a manufactory is already established, we would be far from advising the magistrate to order its removal: it would be sufficient in such cases, to oblige the manufacturer to build very high chimneys, that the disagreeable vapours produced in these operations may be dissipated in the air. This is particularly practicable for the fabrication of Prussian blue, and by adopting it one of our number has continued to retain in the midst of Paris one of the most important manufactories of this kind we have, against which the neighbours had already leagued.

Few injurious to health.

In the report we lay before the class we have thought it our duty to attend only to the principal manufactories, against which violent clamours have been raised at divers times and places. It is easy to see, from what has been said, that there are but few the vicinity of which is injurious to health.

Caution to magistrates.

Hence we cannot too strongly exhort those magistrates who have the health and safety of the public committed to their charge, to disregard the unfounded complaints, which, too frequently brought against different establishments, daily threaten the prosperity of the honest manufacturer, check the progress of industry, and endanger the fate of art itself.

They should not listen too readily to complaints.

The magistrate ought to be on his guard against the proceedings of a restless or jealous neighbour; he should carefully distinguish what is only disagreeable or inconvenient from what is dangerous or injurious; he should recollect that the use of pit-coal was long prescribed, under the frivolous pretence that it was injurious to health; in short, he should be fully aware of this truth, that, by listening to complaints of this nature, not only would the establishment of several useful arts in France be

be prevented, but we should insensibly drive out of our cities the farriers, carpenters, joiners, brasiers, coopers, founders, weavers, and all whose occupation is more or less disagreeable to their neighbours. For certainly the employments just named are more unpleasant to live near than the manufactories mentioned above, and the only advantage they enjoy is that of ancient practice. This right of toleration has been established by time and necessity; let us not doubt therefore, but our manufactures, when grown older and better known, will peaceably enjoy the same advantage in society; in the mean time we are of opinion, that the class ought to avail itself of this circumstance, to put them in a particular manner under the protection of government, and declare publicly, that the manufactures of acids, sal ammoniac, Prussian blue, sugar of lead, white lead, starch, beer, and leather, as well as slaughter-houses, are not injurious to the health of the vicinity, when they are properly conducted.

Disagreeable occupations sanctioned by time.

Manufactures not injurious to health.

We cannot say as much for the steeping of hemp, making catgut, laystalls, and in general establishments where a large quantity of animal or vegetable matter is subjected to humid putrefaction. In all these cases, beside the disagreeable smell they exhale, miasmata, more or less deleterious, are evolved.

Injurious manufactures.

We must add, that, though the manufactories of which we have already spoken, and which we have considered as not injurious to the health of the neighbourhood, ought not to be removed, yet administration should be requested to watch over them strictly, and consult with well-informed persons for prescribing to the conductors the most proper measures for preventing their smoke and smell from being diffused in the vicinity. This end may be attained by improving the processes of the manufactures, raising the outer walls, so that the vapours may not be diffused among the neighbours; improving the management of the fires, which may be done to such a point, that all the smoke shall be burnt in the fire-place, or deposited in the tunnels of long chimneys; and maintaining the utmost cleanliness in the manufactories, so that nothing shall be left to putrify in them, and all the refuse capable of fermentation be lost in deep wells, and prevented from any way incommoding the neighbours.

Manufactures not injurious require some restrictions.

We shall observe too, that when new manufactories of Prussian blue, sal ammoniac, leather, starch, or any other article

New manufactories.

ticle by which vapours very inconvenient to the neighbours, or danger of fire or explosions are to be established, it would be wise, just, and prudent, to lay down as a principle, that they are not to be admitted into cities, or near dwellings, without special authority; and that, if persons neglect to comply with this indispensable condition, their manufactories may be ordered to be removed without any indemnification.

Summary. It follows from our report, 1st, that catgut manufactories, laystalls, steeping of hemp, and every establishment in which animal or vegetable matters are heaped together to putrify in large quantities, are injurious to health, and ought to be remote from towns and every dwelling house: 2dly, that manufactories where disagreeable smells are occasioned through the action of fire, as in the making of acids, Prussian blue, and sal ammoniac, are dangerous to the neighbours only from want of due precautions, and that the care of government should extend only to an active and enlightened superintendance, having for its objects the improvement of their processes and of the management of the fire, and the maintenance of cleanliness: 3dly, that it would be worthy a good and wise government, to make regulations prohibiting the future establishment of any manufacture, the vicinity of which is attended with any essential inconvenience or danger, in towns or near dwelling-houses, without special authority previously obtained. In this class may be comprised the manufactories of poudrette, leather, and starch; foundries, melting houses for tallow, slaughter houses, rag warehouses, manufactories of Prussian blue, varnish, glue and sal ammoniac, potteries, &c.

Such are the conclusions which we have the honour to lay before the class,* and addressed to government, with invitation to make it the base of its decisions.

* These conclusions were adopted by the Institute.

XI.

Facts relative to the Torpid State of the North American Alligator. By BENJAMIN SMITH BARTON, M. D. *.

IT has not, I think, been remarked by the generality of the writers on natural hiſtory, that the North American Alligator paſſes during the prevalence of cold weather, into the torpid ſtate. This however, is unqueſtionably the caſe in ſome parts of the continent.

Mr. Boſſu, a French writer, after telling us that theſe animals are numerous in the Red River, one of the weſtern branches of the Miſſiſſippi, ſays, " they are torpid during the cold weather, and lie in the mud with their mouths open, into which the fiſh enter as into a funnel, and neither advance nor go back. The Indians then get upon their backs, and kill them by ſtriking their heads with hatchets, and this is a kind of diverſion for them †. (Account by by Boſſu.)

Dr. Foſter, the tranſlator of the work, obſerves in the preceding paſſage, " that the circumſtance of the alligator's being torpid during winter is quite new, and very remarkable for natural hiſtory." It ſeems (he adds) almoſt all the claſs of animals called *amphibia*, by Dr. Linnæus, when found in cold climates grow torpid during winter.

In addition to the authority of Mr. Boſſu, I may here mention the following fact, which was communicated to me about the year 1785, by a Mr. Graham, at that time a very intelligent ſtudent of medicine in the Univerſity of Pennſylvania. (Another account by Mr. Graham.)

" The alligator having previouſly ſwallowed a number of pine-knots, retires to his hole, where he remains in a torpid ſtate, during the ſeverity of winter. If killed at this ſeaſon, theſe knots are found highly poliſhed by their trituration one againſt the other in the animal's ſtomach, as I have more than once heard from men of undoubted veracity, who had (The alligator ſwallows pine-knots previous to becoming torpid.)

* From " the Philadelphia Medical and Phyſical Journal". Collected and arranged by Ben. Smith Barton, M. D. It is publiſhed in half yearly Numbers, the firſt of which appeared in November, 1804.

† Travels through that part of North America formerly called Louiſiana. Engliſh Tranſlation, Vol. I. p. 367. London 1771.

been

been concerned in the fact. Indeed this is so notorious in those parts in which these creatures abound, that the digestion of the alligator's stomach is proverbial amongst the multitude, who deride its insipidity in the choice of such food, though, I presume, this it does instinctively, for some purpose unaccounted for by naturalists; and which, perhaps is beyond the limits of human ken."

The fact related by Mr. Graham, relates to the alligator of the Carolinas, in which parts of the United States this animal is very common. By another gentleman I have been informed, that the pine knots which the alligators swallow are generally such as are very abundant in turpentine. I have also been assured, by my friend Mr. William Bartram, that he has seen a brick-bat which was taken out of the stomach of an alligator, and that it was worn quite round.

Local situation of this animal, &c.

Mr. Lawson says, that the alligator is not seen to the North of North-Carolina. They are very common at Cape-Fear in latitude 34. One twelve feet in length has been seen at this place. On the Atlantic side of the United States I am not able to trace them farther than the "Alligator Dismal Swamp," which is between Edenton and Newbern in North-Carolina. The mouth of the Red River in latitude 31.

Within the tract of country just mentioned, the alligator obeying the impulse of the climate, passes into the torpid state. In North-Carolina this takes place about the middle of November, sooner or later, according to the state of the season. Whether the animal becomes torpid in more Southern parts of the Continent, I have not been able to learn. On the river St. John in East Florida, they have been seen awake even in the middle of winter, but it was remarked that they seemed dull and stupid. It has also been observed, that they are accustomed to frequent the warm springs which are so abundant in this part of the Continent; and that they are fond of lying in these springs. Perhaps the heat of these springs may be sufficient to prevent them from becoming torpid. But it must be observed, that a deficiency of heat is not the only cause of the torpid condition of animals.

Conjectures respecting their swallowing the knots of the pine.

It may not perhaps be an easy task to assign a satisfactory cause for the singular instinctive appetite, which leads the alligator, before going into the torpid state, to swallow pine-knots, and other somewhat similar substances. But I apprehend

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prehend that these substances, when taken in by the animal, act in some measure by keeping up a certain degree of action in its stomach, and consequently in every part of the system, and thereby prevents the death of the animal, which might otherwise be destroyed by the long continued application of cold. Some facts mentioned by Dr. Pallas, though they respect a very different family of animals, render this conjecture not a little plausible*.

This subject is worthy of more attention. In particular, it will be well to enquire, whether the alligator does swallow pine-knots, stones, &c. in those parts of America in which it does not pass into the torpid state.

XII.

Observations and Experiments on the conducting Power of Fluids. By T. S. Traill, *M. D. From the Author.*

To Mr. NICHOLSON.

SIR, *Liverpool, Sept.* 10, 1805.

If you think the following observations and experiments worthy of a place in your excellent Journal, your inserting them will oblige,

Your obedient servant,

J. S. TRAILL, *M. D.*

Doctrine of Count Rumford, that fluids are non-conductors of heat, controverted.

Count Rumford was the first who maintained that fluids are absolute non-conductors of caloric. This conclusion he drew from the interesting fact he had discovered, of the extreme slowness with which ice melted when a stratum of cold water was interposed between it and the heated body. He imagines that it was always melted in such circumstances, either by *currents* produced, in some of them by changes in specific gravity, or by the transmission of caloric through the sides of the containing vessel. The experiments of this illustrious philosopher have roused the attention of the learned, and to the united labours of yourself, of Thomson, of Dalton,

* Historia Glirium, &c.

and

and of Murray, we are indebted for an investigation of the Count's opinions, the result of which seems to be, that fluids are not absolute *non-conductors* of caloric.

Experiments of Dalton, Thomson, Nicholson, and Murray, considered by the Count.

The experiments of Dalton and Thomson have proved, that the appearances of currents, such as described by Rumford, may be often illusory; and from those of Nicholson, and from Murray's first experiments, we have strong reasons for supposing, that the temperature was affected by the conducting power of the fluids employed; but in my opinion the experiments of Murray with a cylinder of ice, are the most complete demonstration of this contested point. In a late paper, inserted in the Transactions of the Royal Society of London, Count Rumford endeavours to obviate these objections to his hypothesis, in his usual ingenious manner.

It is not apprehended that the experiment of Murray could be affected by current.

Even admitting that in your experiments the caloric was transmitted solely by the containing vessel (an opinion by no means probable), and that *currents*, such as Rumford describes, have all the effect he attributes to them in certain cases; still the experiments of Mr. Murray appear to me incontrovertible. It was not, therefore, without surprise, that I observed him use the following argument to invalidate their results: "When that vessel is constructed of ice, the flowing "down of the water, resulting from the thawing of the ice, "will cause motions in the liquid, and consequently inaccu-"racies of still greater moment;" viz. than those produced by the conducting powers of the sides of the vessel. Now the melting of the ice could affect the thermometer only by being itself heated, and then trickling down the sides of the cylinder of ice. But I apprehend, the water resulting from the melting of the ice could not gain a higher temperature than 32° F. while it remained in contact with ice. If we mix even equal parts of water at 172° and ice, we do not find that the temperature of the mixture is above 32°. If such a large quantity of water cannot maintain its temperature in contact with ice, can we suppose that such a small quantity as was formed could rise to a higher while trickling down the sides of a thick cylinder of ice.

and certainly not that with mercury in a vessel of ice.

But even this explanation of the phenomenon advanced by Count Rumford, is entirely inapplicable to the experiment with mercury; for the drops of water formed could not possibly sink in a fluid so much more dense, nor throw it into currents which could reach the thermometer.

Besides

Besides those most ingenious experiments devised by Murray, we have other proofs of the conducting power of liquids in several well known facts.

Proofs that fluids are proper conductors.

1*st*. If the *non-conducting* power of liquids have any meaning, it must signify that their particles are incapable of communicating to each other the temperature they have acquired by physical contact with some other body, whose temperature was elevated. If this were true, how shall we account for the mean temperature produced by mixing equal quantities of hot and cold water? Rumford, if I recollect aright, has endeavoured to obviate this objection to his hypothesis by supposing, that it is only an intimate mixture of hot and cold particles which takes place in such cases. If this were true, we should expect, from the rapid motion he supposes the currents to have in liquids that are heated, that they would soon separate into warmer and colder strata, from the difference in their specific gravities: This however is not the case: The whole acquires a uniform temperature.

1. They take a common temperature on mixing.

2*d*. When mercury and water at different temperatures are mixed, an interchange of caloric takes place. From the very great difference of their specific gravities we cannot suppose that every particle of the one has been in contact with every particle of the other; yet they soon acquire a common temperature, which though not a mean, has always a constant relation to the temperature of the two fluids before mixture. Does not this indicate a considerable conducting power in those liquids? Indeed, I cannot conceive that any interchange of temperature could take place in such cases, if the particles of the liquids were incapable of communicating their caloric to the next particles.

2. More particularly water and mercury.

3*rd*. The beautiful experiment devised by Rumford, in which water, in a glass tube, was made to boil over a cake of ice, by the application of a heated body to the upper part of the containing tube, without, for a very long time, affecting the ice, is a sufficient proof of the slowness with which glass transmits caloric, and clearly indicates that the sides of the vessel in several o the experiments of the above-mentioned philosophers, could not be the sole conducting medium.

3. The vessel is too bad a conductor to account for the effects urged against the doctrine of C. R.

4*th*. The sixteenth experiment in Rumford's seventh essay, affords another argument against his opinion. He poured boiling water on a stratum of cold water, which rested on a cake

4. Hot water poured on cold does not raise its temperature by currents.

cake of ice in the bottom of a jar, he found that near the surface of the ice the temperature was 40°, at the distance of three inches it was 159°, but at the distance of seven inches it was only 160°. Had the cold water acquired its elevation of temperature by the currents produced, or by the sides of the vessel, we ought, I apprehend, to have found the temperature spreading more uniformly: but though the first four inches only differ by one degree, we find the next three differing by 119 degrees.

5. Heat applied to fluids downwards.

5th. If liquids were absolute non-conductors of caloric, it would necessarily follow, that when caloric was applied to the upper surface of different liquids, other circumstances remaining unchanged, and provided the liquid did not increase in specific gravity by cooling, *equal increments of temperature would take place in equal times.*

Apparatus for experiments of transmitting heat downwards through fluids.

From several experiments it is probable, that some liquids conduct caloric more rapidly than others. The following were undertaken with a view to ascertain more accurately this point: How far I have succeeded I leave you to judge:

A cylindrical vessel was turned out of wood, having its sides 0.5 inch thick; its height four inches, and its diameter two. It has a moveable wooden top or cover perforated with a hole in its centre a little more than an inch in diameter, into which an iron cylinder of one inch in diameter could be easily introduced. This cylinder is supported by a slight flanch or shoulder-piece, and can be taken up by means of a string attached to its top. When the iron bar is in its place, its flat lower extremity is 0.5 inch distant from the bulb of a delicate mercurial thermometer D L, which is fixed by wax, in a hole perforating the cylinder near its bottom. This thermometer, which was made by the late Ramsden, has a tube as fine as a human hair, and is bended to a right angle, so that its bulb and part of its stem lie in the axis of the wooden cylinder. This shape was preferred, because the stem could be little affected by the caloric transmitted by the sides of the vessel till after the bulb was acted on by the caloric of the iron bar. A variety of experiments were performed with this apparatus in the following manner: The temperature of the room being steadily 67° F. during the trials, a kettle of water was kept boiling over the fire: Its temperature was between 211° and 212°, and into this the cylinder of iron was suffered to remain,

Into various liquids in succession at 67° F. a cylinder of metal at 212°

main, at each experiment, for 15 minutes. The liquid to be examined, and all the apparatus (but the iron bar), were, at each experiment, ascertained to be at 67°. The liquid was poured into the wooden vessel, till it could rise 0.1 inch on the side of the iron cylinder when in its place: The wooden top was put on, and the iron was drawn out of the kettle of boiling water by means of the attached string, and instantly let down through the hole of the cover. The time the thermometer took to rise through three degrees (to 70°) was accurately marked by means of a stop-watch, and the results of my experiments on several fluids are exhibited in the following

and the rise of thermometer 0.5 inch beneath, was observed in three minutes.

TABLE.

Table of results.

	Liquids.	Minutes.	Seconds.
1.	Water, - - - - -	7	5
2.	Milk of a Cow, - -	8	25
3.	Proof Spirit, - - - -	8 nearly	
4.	Alkohol. London Pharm.	10	45
5.	Transparent Olive Oil, -	9	50
6.	Mercury, - - - -	0	15
7.	Solution of Sulphate of Iron, one part of Salt to five of Water, - - - -	8	0
8.	Saturated Solution of Sulphate of Alumine, -	9	40
9.	Ditto Solution of Sulphate of Soda, - - - -	6	30
10.	Aqua Potass. Puræ. Lond. Pharm. - - - -	8	15
11.	Saturated Solution of Sulphate of Soda, but the Liquid not touching the Iron Cylinder by 0.1 Inch, or *nearly so*, -	19	20

As the water in the first experiment was employed at a temperature above 42°, it could not affect the thermometer by any change of density; it may therefore serve as a standard to compare the other liquids. With regard to the differences of a few seconds, we need not insist on it as indicating any material difference between the conducting power of the different substances; because the eye may not be able to mark it instantaneously: but where this difference amounts to nearly a quarter

The temperature was always too high to produce a descending current in water by heating.

a quarter of a minute, much more when, to several minutes, we may fairly conclude, that there is a difference in conducting power.

In all these experiments the sides of the apparatus should have produced equal increments, had this been the cause of the rise of the thermometer; and it is evident that currents downwards could not affect it. That the sides of the vessel could not communicate the temperature to the thermometer, nor even the radiant caloric affect it in the manner observed, the eleventh experiment (which by the way arose from an error in the mode of conducting the trial with sulphate of soda) sufficiently demonstrates. From an inspection of the table, it will be seen, that the aqueous solutions of different salts differ materially from each other in the celerity with which caloric is propagated through them.

I attempted to measure the conducting powers of several of the weaker acids, but I was soon convinced that their action on the iron might invalidate the accuracy of the results.

The fluids are proper conductors.

It will be unnecessary to observe that if we find the thermometer requiring different times for its elevation, in such cases, we must ascribe it to the conducting powers of the medium between it and the heated body.

If I am not deceived, we may conclude from what I have above adduced, that liquids as well as solids are conductors of caloric; that the transmission of it through them follows a particular law depending on the properties of the particular liquid, but which is not in the exact ratio of any of their mechanical properties, though nearer that of their *density* than any other.

The Count's facts may be as well explained by the slow conducting energy of fluids as by its negation.

Very slow currents will explain his fact of the Glaciere of Chamouni.

Such, Sir, are the principal arguments that seem to militate against Count Rumford's hypothesis, which he has, with that ingenuity which distinguishes his researches, applied to the solution of many important phenomena of nature. These, however, may be equally well explained by supposing liquids very bad conductors of caloric; and, if the currents caused in liquids by changes in temperature, have even a very inferior velocity to what he supposes, we may, I think, account sufficiently well for the appearance he observed on the Glaciere of Chamouni, which he proposes as a test of his opinions, by the decrease in density of water while its temperature descends from 42° to 32°, (a fact which the Count's late experiments confirm) without assenting to his opinions with regard

to

to the non-conducting power of fluids. An examination of this would, however, extend farther this already too long letter; but if you deem such an enquiry interesting, it may be the subject of a future communication.

I am, Sir,

Your's with respect,

T. S. TRAILL.

*** As this letter did not come to hand till above a fortnight after its date, and the verbal description is very clear, it was not thought necessary to postpone it for engraving the author's sketch.—N.

XIII.

Indian Account of a remarkably strong and ferocious Beast, which (they say) existed in the northern Parts of the State of New York about two hundred Years ago. Collected and communicated * *by* Mr. JOHN HECKEWELDER.

THE jagisho † (or naked animal, or bear, as some of the Indians call it) was an animal much superior in size to the largest bear. It was remarkably long-bodied, broad down its shoulders, but thin, or narrow at its hind legs, or just at the termination of the body. It had a large head and a frightful look. Its legs were short and thick. Its paws (the toes of which were furnished with long nails or claws, nearly as long as an Indian's finger) spread very wide. Except the head, the neck, and the hinder parts of its legs, in all which places the hair was very long, the jagisho was almost naked of hair; on which account the Indians gave it the name of "naked." Account of the large animal called jagisho by the American Indians.

Several of these animals had before this time been destroyed by the Indians, but this particular one had, from time to time, destroyed many of the Indians, particularly women and children, when they were out in the woods getting nuts, digging roots, &c. or when they were working in the fields. Hunters when fast pursued by this animal, had no means of escaping

* To the Editor of the Philadelphia Medical and Physical Journal, whence this is taken.

† The Indian name of this beast or animal.

from

Account of the large animal called Jagisho by the American Indians.

from it except where a river or lake was at hand, by plunging into the waters, and swimming *out*, or *down* the stream to a great distance, they effected their escape. When this was the case, and the beast was not able to pursue his intended prey any further, he would set up such a roaring noise, that every Indian who heard it trembled with fear.

This animal preyed upon every beast it could lay hold of. It would catch and kill the largest bear, and devour it. While the bears were plentiful the Indians had not so much cause to dread the jagisho; but when this was not the case, he would run about in the woods, searching for the track or scent of the hunters, and follow them up. The women became so much afraid of going out to work, that the men assembled to deliberate on a plan for killing him.

This beast had its residence at or near a lake, from which the water flows in two different ways (or has two different outlets), one northerly and the other southerly. The Indians being well informed of this circumstance, a resolute party of them, well armed with bows, arrows, and spears, made towards the lake. They stationed themselves on a high perpendicular rock, climbing up the same by means of Indian ladders, and then drawing these ladders up after them.

After being well fixed, and having taken up with them a number of stones, the Indians began to imitate the voices and cries of the various beasts of the woods, and even those of children, in order to decoy the jagisho thither. Having spent some days in this place without success, a detached party took an excursion to some distance from the rock. Before they had reached the rock again, the beast had gotten the scent of them, and was in full pursuit of them. They, however, regained that position before he arrived. When he came to the rock, he was in great anger, sprang against it with his mouth wide open, grinning and seizing upon it, as though he would tear it to pieces * * * *. During this time, numbers of arrows and stones were discharged at him, until at length he dropped down and expired.

His head was cut off, and was carried in triumph by the Indians to their villages or settlements on the North River, and was there fixed upon a pole that it might be seen. As the report of the death of the animal spread among the neighbouring

neighbouring tribes, numbers of them came to view the head and to praise the victorious Indians for their warlike deed.

Account of the large animal called jagisho by the American Indians.

N. B. The Mahicanni claim the honour of this act.

REMARKS BY THE EDITOR.

The preceding traditional accounts of the Indians, concerning the "naked beast," are in some respects, so very extravagant, that they may perhaps be deemed altogether unworthy of any attention. I must confess, however, that I cannot but consider such traditions, though imperfectly handed down to us, and evidently disfigured by fable, as entitled to the notice of the naturalist and philosopher.

That such an animal as the Jagisho is described to have been, has ever existed in the state of New York, may perhaps admit of a rational doubt; but that the Indian tradition relates to *some remarkable* animal that is no longer to be seen in the country which it is said to have inhabited, I think there is good reason to believe. What this animal was, at what period it ceased to be seen, and what was the more pure account of the Indians concerning it one hundred years ago, I do not pretend to determine.

Possibly the Indian tradition refers to the large animal, (I mean an individul of the same species,) some of whose bones have been found in a cavern in the back parts of Virginia; the animal of which mention is made in the first part of this Journal*. Is is true indeed that the Indian accounts of the activity of the New York animal are not very favourable to the idea, that the animal was Mr. Jefferson's Megalonyx, which I have supposed belonged to the order of Tadigrada, comprehending the Sloth, the Armadillo, and others. What is said of the claws of the Jagisho may be thought to favour the notion that this was really the Megalonyx, or Megatherium. But I would not be understood to place any dependance upon the minute or descriptive circumstances which are mentioned in the Indian tradition. Nor indeed do I think it at all probable that the Megalonyx (as it is called) or any of the species of elephants whose exuviæ abound in various parts of North America, have been seen in a living state in this Continent, within the period of two, or even twice two hundred years.

* Section Third, p. 152, 154.

SCIENTIFIC NEWS, &c.

Death of Professor Claproth.

Klaproth.

SOME of the foreign Journals have announced the death of the celebrated Klaproth of Berlin, who, for the benefit of the Sciences, continues in good health in his sixty-second year. Mr. Justus Claproth, Professor of Jurisprudence in the University of Gottingen, well known for several learned works on that subject, died on the 10th February last, in his seventy-seventh year.

Astronomical Prize.

Lalande's prize given to Harding.

The medal founded by de Lalande for the best astronomical work, has been adjudged by the National Institute in its sitting of April last, to Mr. Harding, for his discovery of the last new planet. That able astronomer has been appointed to the direction of the Observatory at Gottingen.

New Musical Instrument.

New musical instrument.

A Polish clock-maker named Matlousky, arrived at Berlin, at the beginning of the present year, with the intention of exhibiting a new stringed instrument, of his invention. Notwithstanding a variety of advertisements, he did not succeed in attracting the public notice; and he determined to exhibit the instrument at a concert previous to his departure. About 300 auditors attracted by the names of Himmel and Seidlen, who were to perform, attended, and towards the end of the concert nearly half the number retired. The artist proceeded to exhibit his Kalison, which is the name he gives the instrument. It consists of a sound-board, on which the usual system of wires of the piano are fixed. Between these wires are small wooden cylinders, which being put into motion, communicate their vibrations to the wires. The tones are so soft and enchanting, that the harmonica cannot equal them, the forte and piano are given in every imaginable gradation, and the whole effect was no less surprising than unexpected, and the maker Huhn received orders for a number of the instruments.

The

The present article is taken from Millin's *Magasin Encyclopedique*, who does not say whether the wooden cylinders were moved in rotation or otherwise, nor how they were applied and pressed against the strings. The leading fact of this notice seems to be, that there are certain kinds of wood, and perhaps certain resinous or other matters to be applied to them, that will produce the effect of a bow upon wire strings in a superior manner. It is indeed probable, that we do not yet possess much knowledge of the art of producing tones by the powerful expedient of bowing, or light friction; and mechanics have still an ample field for applying this method with force, precision, and rapidity to the more compounded instruments.

Saverien.

On the 28th of May last died Alexander Saverien engineer of the French marine; who has been sixty years known to the scientific world, for his writings on navigation and the theory of building, rigging and manœuvering ships. He has written accounts of the instruments for making observations at sea; a marine dictionary; a dictionary of the mathematics; a dictionary of architecture; an history of modern philosophers, and an history of the progress of the human understanding. His works indicate a considerable share of talent and very extensive knowledge. For many of the last years of his life he was poor and infirm, and was much indebted to the cares of a servant who continued with him from motives of attachment. He died at the age of eighty-five, leaving behind him a widow likewise very aged and in want. Death of Saverien.

Pure and beautiful Ceruse.

Mr. Van Mons informs us, that, if lead ashes be dissolved in a sufficient quantity of dilute nitric acid by the help of a gentle heat, filtered, and precipitated by chalk in impalpable powder, the precipitate, when washed and dried, will be the purest and most beautiful ceruse possible. The question which offers itself on this occasion, is whether it could be afforded at a reasonable price. Pure and beautiful ceruse.

Chromate of Lead and of Silver.

Chromate of lead and of silver dissolved in nitric acid.

Count Mouffin Poufchkin has diffolved both the red lead fpar and chromate of filver in nitric acid, by adding a little fugar the moment the acid is poured on, and promoting the action by gentle heat. The fpar then requires only five or fix parts of acid, the chromate of filver ftill lefs. Nitrous acid gas is evolved, and the folutions of the former is an amethyft colour, of the latter a garnet red, without the leaft trace of green, either by reflection or refraction.

Putrefaction prevented.

Putrefaction prevented by red precipitate,

Dr. Valli having left a pound of foup in which were twelve or fifteen grains of red precipitate, expofed to the open air for four months, found it exhibited no fign of putrefaction, and did not even feem to have undergone any alteration. He then repeated the experiment for a month in the height of fummer, with the fame effect. The oxide in the mean time had neither diminifhed in weight nor altered its colour.

without lofs of weight or colour;

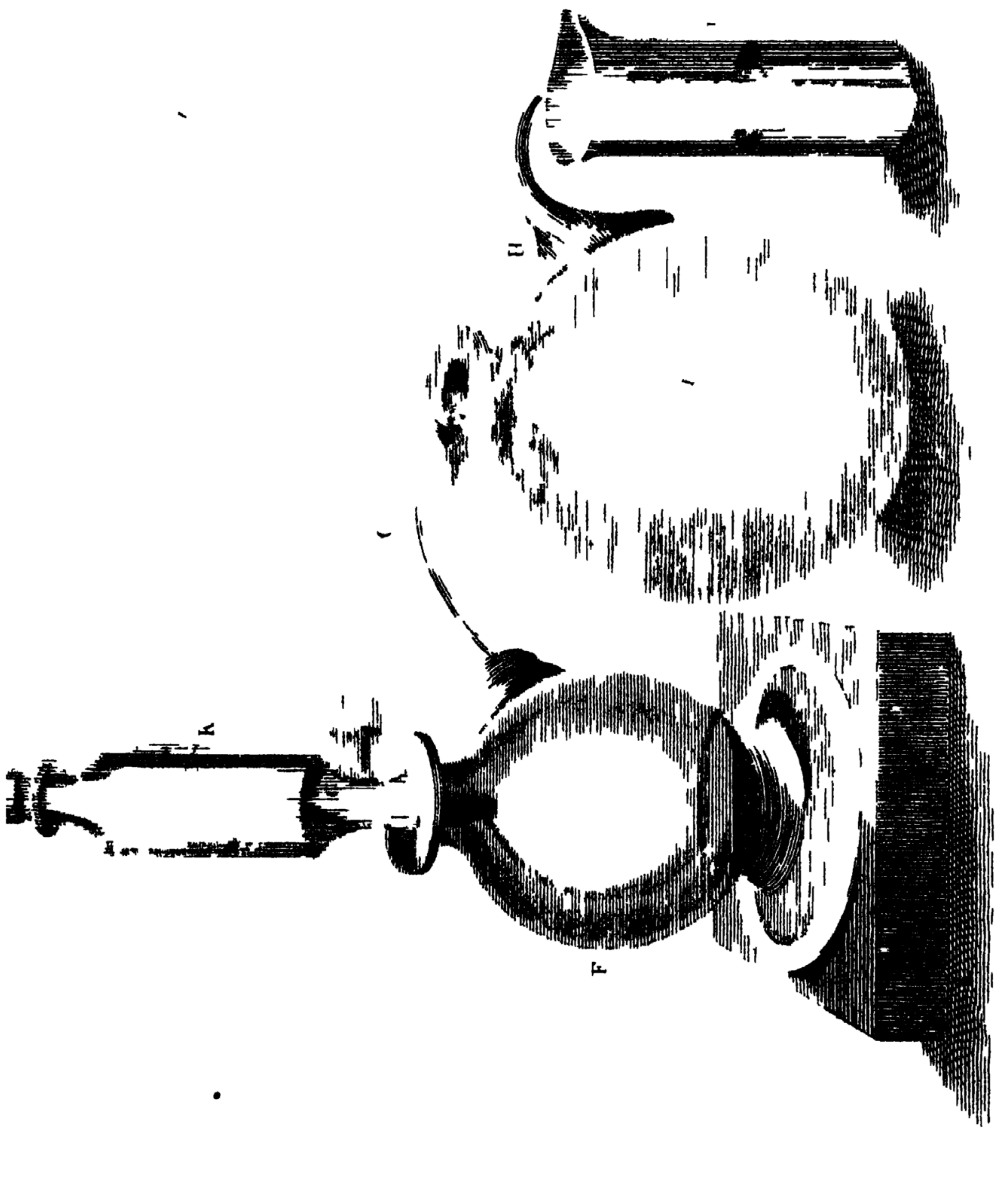
and by the combination of oxigen.

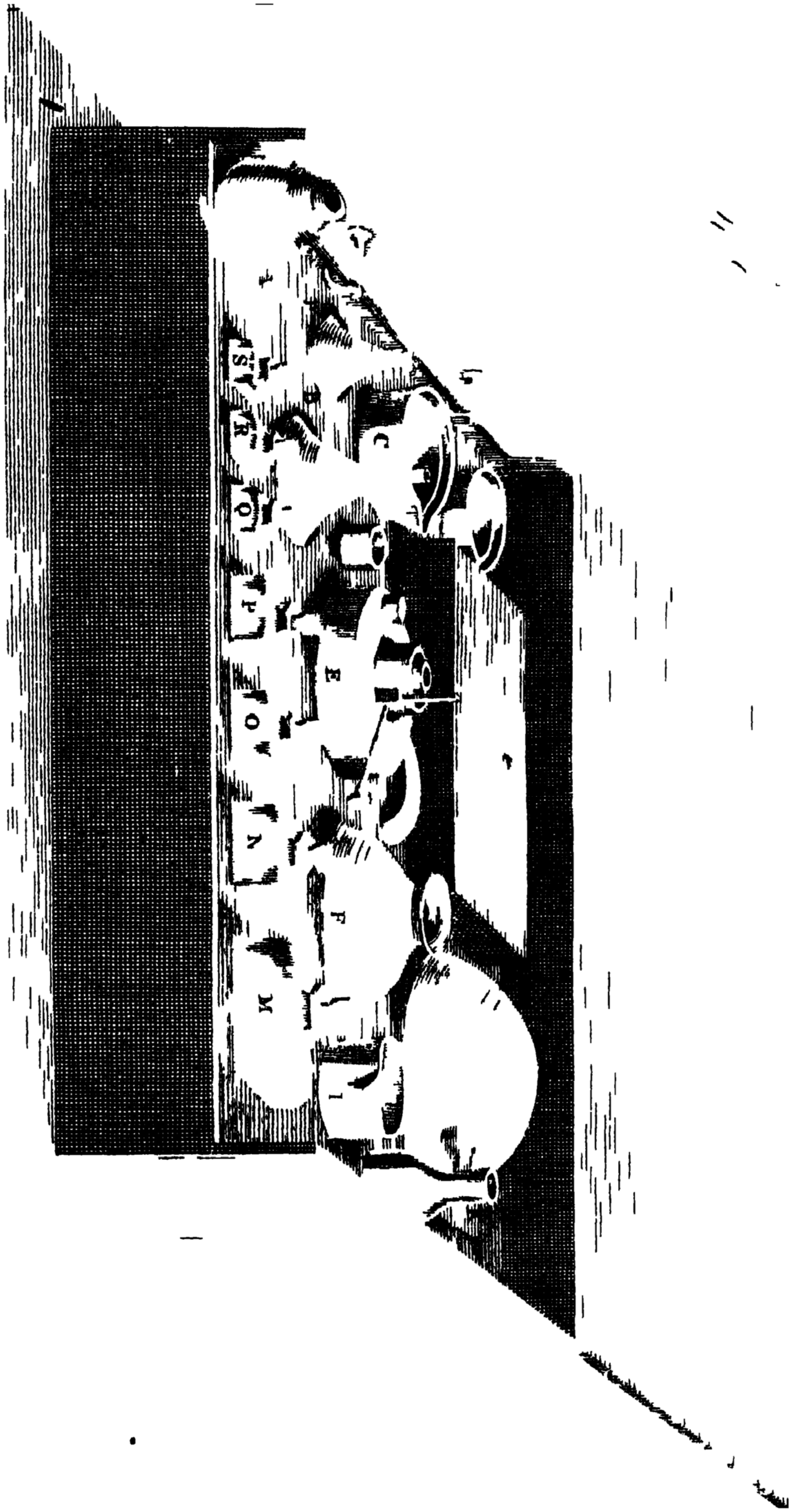
On this Mr. Van Mons obferves, that he has found broth keep for years by means of a few grains of mercury in the ftate of oxide and citrate. Nitrate of filver has long been confidered as the moft powerful of antifeptics, and he has found thofe of gold and of mercury equally fo. Oxigenated muriate of potafh retarded the putrefaction of ftrong foup feveral days, and ultimately put a ftop to it at a certain point. Very dilute nitric acid, and oxigenated muriatic acid in the ftate ufed for bleaching, preferved a moderate ftrong foup for feveral months.

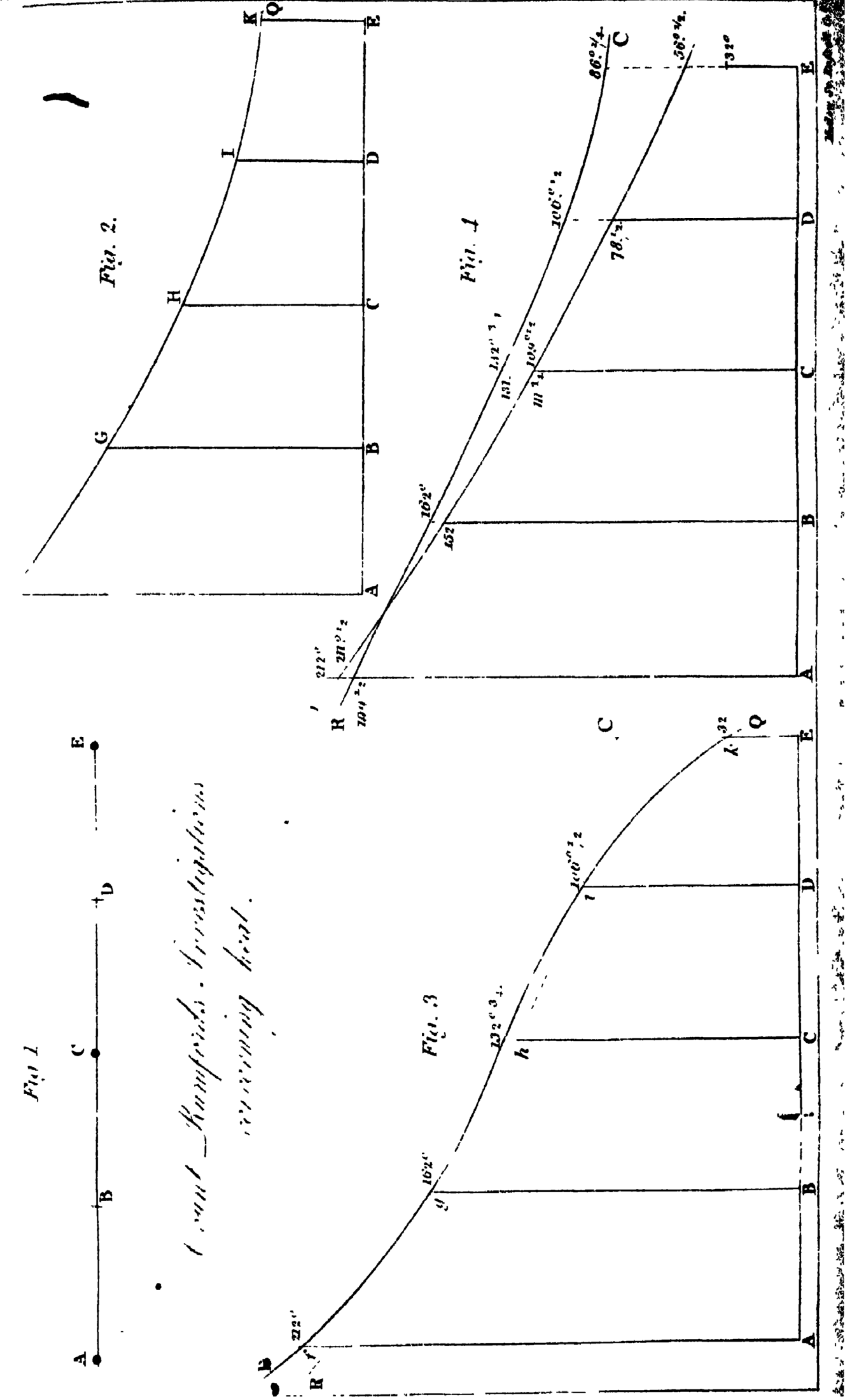
Leverian Mufeum.

Sale of the Leverian Mufeum.

The Leverian Mufeum which has been near 40 years in collecting at an expence of near 50,000*l*. will be fold by public auction in May 1806, unlefs an acceptable offer for the purchafe of the whole be previoufly made. The fale will take place in the building which now contains the Mufeum. Catalogues are preparing with all fpeed.



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JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1805.

ARTICLE I.

Facts and Observations relating to the Blight, and other Diseases of Corn. In a Letter from G. CUMBERLAND, *Esq.*

To MR. NICHOLSON,

SIR, *Weston-supra-Mare, 10th October*, 1805.

ALTHOUGH not much in the habit of taking any thing on trust, I must confess, that when early in the spring the pamphlet of Sir Joseph Banks, called, *A short Account of the Cause of the Disease in Corn, called by the Farmers, the Blight, the Mildew, and the Rust* *, came to my knowledge, I felt very much inclined on the credit of his extensive fame to receive with a favourable prejudice, what was so positively announced as information, on a subject that we may all, I suppose, be allowed to have something to do with.

Reference to the Pamphlet of Sir Joseph Banks on the blight in corn.

Having therefore much leisure, and being placed in a favourable situation to make observations, I began doing all that could then be done, viz. planting some wheat near a barberry bush, and searching carefully for the yellow rust on the early leaves.

Corn sowed near a barberry bush.

* Inserted intire in our Journal X. p. 225.

Diseased straw being examined, it was seen that Mr. Bauer's drawings are very correct.

I next examined the straw by the microscope, making many dissections with great attention. The result of which was, a conviction that the designers and engravers part of the fine plates that accompany this essay, has been excellently and faithfully managed, as far as the then season would permit me to compare them, and some old straws of the last year rendered it very probable, that the whole was correctly drawn.

Doubt whether this disease affects the grain.

But here I began to suspect, that the *rust* was not so guilty as has been represented, for, admitting for a moment, that it does intercept the sap by plugging the apertures said to be destined in wet weather to receive the humidity of the atmosphere, yet, as it is *not yet ascertained* that it strikes root, into the cellular texture beyond the back, for I could not admit of saying there is *no doubt* of a thing that *has not yet been traced*, see page 11 (or 227 of Journal) and as the plates, if they prove any thing *prove the contrary*, *vide* fig. 7. I thought we must wait awhile before we could charge to this cause the diminution of our flour. There was yet another motive in my humble opinion for doubt: I saw, even by examining the straws of last year, that it was scarcely possible to find among hundreds of *rusted straws* that had *blighted heads*, one that any way partook of the rust except at the upper joint, and that partially only, while the sheaths that nature has kindly given to ward off injuries, were compleatly consumed with it. Now I believe, no one will pretend to assert, that this injury done to the sheaths of the straws could in any way affect the rising of the *sap* to the ear; we must therefore, I soon saw, confine the cause of injury, *if this be any cause*, to the quantity of fungi that more immediately attacks the upper bare joint of the straw.

And even here it appeared at this early stage of my doubts to be very uncertain; for if we reflect first of all that it is by no means pretended to be proved, that these fungi do penetrate the cellular texture; and next, that if they do, it will remain to be proved, that by so doing they materially intercept the sap; and lastly (which I conceive to be no extravagant conjecture) that if they did, yet as far as the cellular texture of the straw is concerned in conveying it, the interception could only be very partial.—Taking all these reflections together, I think I was grounded in entertaining doubts of the true *cause* having been exhibited, as is set forth in this pamphlet, p. 13.

p. 13. (or 228 Journal.) Where the president says, " though diligent enquiry was made during the last autumn, no information of importance relative to the origin or the progress of the *blight* could be obtained! this is not to be wondered at, for as no one of the persons applied to had any knowledge of the *real cause* of the malady, none of them could direct their curiosity to the real channel. *Now that its nature and cause have been explained*, we may reasonably expect that a few years will produce an interesting collection of facts and observations, and we may hope that some progress will be made towards the very desirable attainment of either a preventive or a cure." page 14.

Having thus advanced in the examination as far as the season would afford, I thought it would be best to ascertain in the county in which I happened to be placed, the terms (intirely overlooked in the pamphlet) there applied to the different diseases of corn; and here it soon appeared that the terms are not universal. Country names of the diseases of wheat.

The first blight (for there are many) is that early appearance of intirely decayed ears, of plants apparently in a healthy state, but which, in the embrio which lays within the upper sheath, before the ear is develloped, has turned to a brown puce-coloured powder. This by some has been supposed to originate from defective seed, but surely improperly; for many of these ears are found to be the finest and largest in the field in their embrio state; and to me it seems evident, that they are imperfect from a really defective parturition, owing to some accidental circumstance; or possibly (for they generally stand below the others) from the want of sun to unclose the upper sheath at a critical moment. First blight; or mildew, by which the embrio grain is turned to powder.

These ears are also in general found to be crooked at the root of the ear stalk, owing to the effort to raise the ear acting within the sheath on a decayed and mouldy ear; but it appears that they are all at length ejected from the sheath, and the brown dust blown compleatly away, so that the stalk at last remains without a single grain on it, standing up like a bare and barren pole. In this manner many ears at first perish, but the quantity is seldom any object to the farmer. The ears are usually crooked at bottom, &c.

Of the puce-coloured powder a quantity was collected: it had no smell, but felt soft to the touch, like whiting, though more greasy. An attempt was made to innoculate a number Appearance of the powder; it produces no infection.

 of

of other found ears with it; by rubbing it on the leaves, by giving friction to the straw itself with it, by inserting it into the ears, by placing it beneath the sheaths, and lastly, by introducing it into the pith of several straws,—each of these in wet and dry weather, but nothing took any effect. It fell harmless:—but one discovery arose from examining the ears in which it was produced, viz. that corn is capable of being completely blighted without any external disease or application. For all the straws were without blemish as well as the leaves, and consequently we have no occasion to recur to external causes for this internal decomposition.

The first blight does not arise from an external cause.

Wheat in this state I drew very accurately, and sent specimens of it to town in this condition to Mr. Nicholson; and thus ended the first blight as it is called in Somersetshire, but which might possibly be with more propriety termed the *mildew*.

Second disease. Smut. Full grains, containing only a dark powder of a fishy smell.

The next disease appears in those ears which stand erect and staring, indicating their lightness by their attitude. Although on gathering they appear full of corn, they turn out in effect to be full of a dark powder that has the smell of stale lobsters or shrimps, when pressed between the fingers. This powder on examination by the microscope shews some saccharine concretions among it, but it has none of the actual properties of wheat. Upon carefully examining these grains the outer skin was discovered to be *intire, unperforated, perfectly green, and perfectly full*;—yet strange to tell, if it be not really the work of insects, one half of an ear was often found to be thus smutty while the other half was found.

Mechanical injury did not produce this disease.

Among other conjectures, it was thought that this smutty disease might arise from the juices of the straw being intercepted by accidents. In order therefore to try what could be effected by injuries done to the sap, I bound some ears, wounded others by pressure, divided some with a knife near the stem, marking each by cutting off the beards with scissars; yet I never found any such effect produced, as either smut, or even decay, and all the ears thus injured came very well to maturity. Here were real injuries, and committed at a time (June) when the ears were by no means far advanced, and this led me still less to expect any great effect from a little partial moss adhering to the stem, the accidental effects of the season at a later period.

At length on the 1st of August, I saw in a low field the first appearance of yellow rust, but collected one with difficulty; and now I found that it was universally agreed by our farmers, that this fungus, as our plate describes it, was considered as the effect of fogs or great humidity, which first attacks the leaf or sheath of the straw, but ultimately penetrates, and vegetates on the upper joint of the straw itself, where it is uncovered just before the corn is ripe, so that what our plate exhibits is by all agreed to be called the *rust* in all stages; but that the rust causes the *latter blight*, or ears with shrivelled grains does not seem to be so generally agreed.

Third disease; the rust considered as effect of humidity.

To prevent the *smut*, our farmers steep their wheat in salt water, in order to separate the sound grains from the light or blighted better than by fresh water, because salt water naturally floats all but the heaviest. In the *Venetian* state I remember they added saltpetre to the steep on the same principle, a very different system from that of Sir Joseph, who recommends, I think very dangerously, the use even of tailings as seed, and this on mere hot house experience, see p. 25 (or 232 Journal.)

The process of steeping grain is used to separate the heaviest for seed.

An old and good farmer * last year at *Weston*, bought good wheat for seed because his own was *smutty*, but not having quite enough, he sowed about three pecks of his own smutty wheat to finish, and it turned out quite as good wheat as some of that which was bought as the best; yet this does not confirm the doctrine, I think.

Smutty grain not used for seed, yet some succeeded.

Having thus ascertained what is called the early *blight or mildew*, together with that which follows, and is known by the term *smut*, and also the disease which comes next, and being similar to Sir Joseph's Banks's plate, is called *rust*, and lastly the *latter blight* which is seen in merely shrivelled grains, or grains imperfectly ripened;—I shall now proceed to the specimens which I gathered in all the states, made drawings of them, and still retain in the ear labelled with great care, as proofs of what is here advanced; after which I shall make some deductions.

Latter blight; or shrivelled grains.

On 22nd of July, 1805. I began my collection; and No. 1. contains healthy ears, clean to outward appearance, the lowest leaf a speck or two of fungus. Second joint from the head a little reddish. No bloom on the cane.

Accounts of specimens of wheat gathered in this enquiry.

* Mr. Oakley of Weston-Supra-Mare.

No. 2.

Accounts of specimens of wheat gathered in this enquiry.

No. 2. Much diseased straw, bloom or mould under the sheath, and on the leaf; in other respects the straw quite as healthy and sound as No. 1.

No. 3. Entirely diseased, yet every grain full and of its proper size—some grains evidently opened at their sides by some small insect. The outside of this ear quite green and healthy, not even a spot, smell of the ear very fishy, not wanting one grain. The head stalk not even waved at the ear, root intire, upper stalk a little yellow.

No. 4. A healthy ear of the bearded, thirteen rows of grains, straw mouldy under the sheath, and at joints, yet sound, last or upper joint green.

No. 5. Another nearly similar.

No. 6. A double straw to one root, both ears perfect, both straight at head stalk, both diseased, yet full and plump; some grains found yet green, and close to a black one; a rich golden coloured moss or dust at the back of the green coat, yet the grain coat perfectly green and uninjured, while the grain was compleatly full of the black fishy-smelling blight instead of flour. Straws green at the top and quite healthy throughout.

No. 7. A straw that having been blighted in the sheath by the early blight, had thrown off all its brown dust, and grown to a strong straw: The skeleton of the ear only remaining, very crooked near the ear: The upper stalk evidently by its purple stripes diseased under the skin, yet no moss or fungus protruded on the cane; stripping back the first leaf from the head, I found the powder from the ear had adhered to the straw under the sheath, and that it was mouldy: The second joint quite healthy under the leaf, but with the red and purple streaks where uncovered to the light: The third joint the same: The sheath leaves themselves healthy.

No. 8. Spotted ear externally, sound straw, yet having black dust at the joints, in this ear I found a maggot about the 30th part of an inch long, of the exact yellow of the powder found behind the diseased grains, (that powder may be his excrement) viz. orange yellow, his form resembled the maggot of nuts, lived an hour on the table.

No. 9. Diseased ear, grains all blighted yet no yellow powder under the sheaths of the grains, straw healthy, ear fishy smell.

No. 10.

No. 10. Ditto with an insect very active in the ear, yellow, see drawing A B. Fig. 1. Plate IX.

No. 11. Two perfectly healthy ears from the same root, both healthy throughout, yet on the leaf a speck or two of Sir Joseph's fungus was originated, and well grown: it was of the orange colour.

No. 12. An ear fast ripening, solid in the grain, yet had lost its first six lower grains, last joint green, beards yellow, a few spots on the sheath of the grain, seemingly occasioned by a small black fly found in it.

Observations made on the same Day on Grains.

Diseased grains described.

Diseased grains were always found to be full and plump without any aperture; the fine skin that holds the flour very green, yet all black within; the external surface of the black matter covered with a white concretion, perhaps the saccharine matter of the wheat. Sound grains found in diseased ears, all the diseased ears smelt fishy.

Observations made on the Straw.

Diseased straw.

The disease always attacks the portion of the straw that peeps beyond the sheath leaf near the joint, and evidently commences at the pores, as old straws will shew; but the fungus cannot, I think, be the cause of the disease; because where no fungi have taken root, the corn is compleatly corrupted. These fungi grow it is true, fastest on diseased ears, probably because when the ear is diseased it draws less humidity from the straw, so that the diseased ears seem to assist the growth of the fungi, not the fungi the disease. In fact, I believe they live on the superfluous moisture of the straw, or returning sap.

Blighted ears, the straw not rusted.

On the 27th July last, 1805. I examined at least twenty blighted ears of corn, and could only find one among them that had the smallest degree of *rust*, and that only a speck or two on the lower leaf that sheaths the third joint. The spots were orange colour and deep purple, and did not occupy half the diameter of a space of three inches long.

Healthy plants having the straw injured.

Upon again examining many *healthy plants*, I found not only their sheath leaves, but even the straw eaten away by

some

some insect, and at the same time discovered abundance of the green locust like insect on the ears; of which, see the magnified design, Fig. 2. Plate IX.

Inoculated plants, and others injured by violence, took no disease.

Some plants that I had lately innoculated were still sound: others that I had pinched and bruised in the upper joint shewed no alteration, but in all respects resembled the most healthy.

Plants gathered which have the straw and the grain not in the same (healthy or diseased) state.

Aug. 3rd, 1805, I went to a field of Mr. Oakley's, o Weston, where the wheat had a general good aspect; and selected and labelled, of ears green yet full.

No. 1. Sound ears with diseased straws.

No. 2. Compleatly smutty ears with sound straws in every respect.

No. 3. A smutty and a sound ear from the same root. The sound ear had a speck of fungus.

No. 4. Ears with crooked tops, others twisted by spiders; others with crooked beards, short stalks and long; yet all of them full of grain, green and sound.

No. 5. Ears half smutty, viz. on one side all the way

No. 6. Ears half stagged, (stagged means here those t shew only the skeleton of the ear and the crooked upper stalk from early blight, all but the bare poles being blown away) these ears were half *stagged* and half covered with grains.

No. 7. Bare *staggs*, but with quite fine sound straw.

Other instances of sound wheat with rusty straw; and shrivelled wheat with sound straw.

Lastly; in August and September, 1805, at Allcombe near Minehead, I collected out of a field just reaped, two bundles which with all the others I still keep as proofs of my assertions: One all of sound wheat with all their upper stalks very much covered with the rust of Sir Joseph Bank's description, and the other all of black and shrivelled ears, yet all sound in the upper stalk.

The shrivelled corn was never fairly ripened.

These latter mentioned stalks, I think, throw great light on my ultimate conjectures drawn from every observation through the whole season; viz. they present shrivelled, blighted grains, and exhibit short ears, because on examination they were evidently never sufficiently exposed to light and heat: for all their straws though clean were green, not yellow as those of ripe wheat ought to be, and their smoaky miserable appearance (not having the least smell of the filthy smutt) could only arise from their humble situation below the other ears, where air, sun, and light was deficient; in fact, they never ripened properly. The straw remained green, and the sap probably

probably returned instead of being intercepted by the drying of the upper joint; and to me it now appears to be a fair conjecture, that what is generally called blighted corn, or those ears that produce so many shrivelled grains, and which we are called upon to seek a remedy for, is nothing more than the effect of a practice of late much recommended, viz. *to reap early*, a practice not only promoted by the *Miller*, who is eager for the new corn to come to market, but by the avarice of the *Farmer*, who fears that by letting it stand too long the grain may fall in price, and reduce his profits; and, what is still more unfortunate, by some agricultural writers of great reputation, who recommend it as stopping the progress of the rust, forgetting that the sun only can effectually destroy that supposed evil, by well drying the straw.

What is called blighted corn seems to be corn prematurely reaped, from avarice or ignorance.

Far be it from the writer of these few remarks to discourage any attempt at saving in a remarkably wet season, or in remarkably wet situations, wheat that has passed the period usually productive of ripe corn. He knows that in cases of laid-wheat in shady situations, by reaping it early, we may accelerate the ripening of that which otherwise would not have ripened at all, by the operation of turning and exposing the sheaves to the sun, and so make good saving crops; but what he wishes to guard against is, that eagerness for putting in the sickle originating in the motives before alledged; for, reasoning from analogy do we not always find, that in all other seeds that are to be gathered, those alone are plump, sound, and full of their proper flour, that are suffered to receive the utmost influence of the sun while on the stalk; and he always thought many years ago, that we were in the habit of being too fearful of the latter seasons; it is true that late harvests are expensive in collecting, but they are generally well matured, and the instance of *barley* that may be well saved (for colour can have little to do with the intrinsic value of grain) even as late as November, proves the justice of the observation; fruit gathered too soon, disappoints all views of profit or pleasure, and we might, he thinks, as well attribute the shrivelling of our apples early gathered to the influence of the apple-tree moss, as the shrivelling of our grains of wheat to a supposed blight originating in the funguses, that have of late so much alarmed the theoretical agriculturists, and œconomists of our day.

It is often adviseable to reap early.

Argument in favour of late reaping.

On

Conclusion.

On the facts exposed I could greatly enlarge, but I think on all accounts it is best at present to offer them in their present form to the reasoning faculties of your readers. At any rate they may serve as data, and if they should fail to bring others to my opinion, may act, I trust, as useful stimulants to the further investigation of a very curious subject of enquiry, as to what are the nature of the enemies to the perfection of our wheat harvest.

With respect and esteem,

I am, Sir,

Your most obedient humble Servant,

G. CUMBERLAND.

The vicinity of the barberry does not seem to affect grain.

P. S. I ought to observe, that on the grains of wheat sown near the barberry, I had no opportunity of making observations; but that I have a dried root of wheat now by me, in which there are above 100 straws that are all clean and sound, though it grew a few years ago in a garden where barberry bushes were.

II.

Experimental Investigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. &c.*

(Concluded from Page 75.)

SECT. III. *Experiments tending to shew that Heat is communicated through solid Bodies, by a Law which is the same as that which would ensue from Radiation between the Particles.*

Object of inquiry; the laws of the propagation of heat in solids.

HAVING made a considerable number of experiments on the passage of heat through fluids, and through different substances in the state of powder, I was curious to ascertain the laws of its propagation through solid bodies, particularly metals.

I hoped this discovery would furnish some additional data, to confirm or refute the opinions I had adopted concerning heat and its manner of acting; and it will be seen by the results, that my expectations were not frustrated.

Having

Having procured two cylindrical veſſels of tin, each ſix inches in diameter and ſix inches high, I faſtened them together by means of a ſolid cylinder of copper ſix inches long and an inch and half in diameter, which was fixed horizontally between the two tin veſſels. The extremities of the cylinder paſſed through two holes an inch and half in diameter, made for the purpoſe in the ſides of the veſſels, midway between the bottom and top, and were ſoldered faſt in them.

Deſcription of an inſtrument: Two cylindrical tin veſſels were connected by a bar of copper.

Each of the veſſels was made flat on the ſide where the copper cylinder was faſtened, ſo that the extremity of the cylinder did not project into the veſſel, but was level with the flattened part.

This inſtrument was ſupported at the height of eight inches and half above the table on which it ſtood, by means of three feet, two fixed to one of the veſſels, and one to the other.

One of theſe veſſels being filled with boiling water, the other with water at the freezing point; as the two extremities of the cylinder were placed in immediate contact with theſe two maſſes of fluid, a change of temperature muſt neceſſarily take place by degrees in all the interior parts of the cylinder. For the purpoſe of obſerving this change, three vertical holes were made in the cylinder, into which were introduced the bulbs of three ſmall mercurial thermometers. One of the holes was in the middle of the cylinder; the others midway between the centre and either extremity.

The veſſels filled with water, one at 212°, the other at 32°.

The changes marked by three thermometers at equal diſtances.

Each of theſe holes is four lines in diameter, and eleven lines and half deep; ſo that the bulbs of the thermometers, which are three lines in diameter, were all in the axis of the cylinder.

When the thermometers were put in their places, the holes were filled with mercury, in order to facilitate the communication of heat from the metal to the bulb of the thermometer.

To keep the hot water conſtantly boiling, a ſpirit lamp was placed beneath the veſſel containing it; and to keep the cold water conſtantly at the temperature of melting ice, freſh portions of ice were added to it from time to time.

The [illegible] water kept boiling by a lamp; and the other cold by addition of ice.

* The thermometers are graduated to Fahrenheit's ſcale, the freezing point being marked 32°, and that of boiling water 212°.

As the firſt and moſt important object I had in view was, to learn at what temperature the three thermometers would become

The thermometers not noticed till nearly ſtationary.

come ſtationary, I did not very carefully notice the progreſs of the thermometers toward this point; but as ſoon as they appeared nearly ſtationary, I obſerved them with the greateſt attention for near half an hour.

The thermometers diſtinguiſhed.

To diſtinguiſh the three thermometers I ſhall call that neareſt the boiling water B, that in the centre C, and that neareſt the cold water D.

Experiment.

The following are the progreſs and reſults of an experiment made the 28th of April, 1804, the temperature of the air being 78° of Fahrenheit,

Tabulated reſults,

Time.			Temperature of the hot water,	Temperature marked by the thermometer B.	Temperature marked by the thermometer C.	Temperature marked by the thermometer D.	Temperature of the cold water.
H.	m.	s.	Degrees,	Degrees.	Degrees.	Degrees.	Degrees,
1	52	15	212	160	130	105	32
—	53	30	—	160¼	131	105½	—
—	55		—	161	131¼	106	—
—	56	30	—	161¾	132	106½	—
—	58		—	162	132½	107	—
2	0	0	—	162	132¾	107½	—
—	1	30	—	162	133	107½	—
—	4		—	162	132¼	106½	—
—	6		—	162	132	106	—
—	9		—	162	132¼	106½	—
—	11		—	162	132¼	106½	—
—	28		—	162	132¼	106½	—

Account of the reſults that would have followed the hypotheſis of heat being propagated through bodies by radiation from particle to particle.

Before I proceed to examine more minutely the reſults of this experiment, I will endeavour to ſhow thoſe reſults which it ought to have exhibited, on the ſuppoſition that heat is propagated, even in the interior of ſolid bodies, by *radiations* emanating from the ſurfaces of the particles compoſing theſe bodies.

The particles of ſolid bodies are diſtant from each other.

On this ſuppoſition we muſt neceſſarily conſider the particles that compoſe bodies as being *ſeparate from each other*, and even to pretty conſiderable diſtances compared with the diameters of theſe particles; but there is nothing repugnant to the admiſſion of this ſuppoſition; on the contrary, there are many phenomena which apparently indicate that all the ſolid bodies with which we are acquainted are thus formed.

To

To see now by what law heat would be propagated in a solid cylinder, let us represent the axis of this cylinder by a right line A E, *Plate* VII. *Fig.* 1; (see our last nnmber) and let us begin with supposing that the cylinder consists of three particles of matter only, A C E, placed at equal distances in that line.

If the solid cylinder consist of three equi-distant particles,

Let us farther suppose, that the extremity A of the cylinder is constantly at the temperature of boiling water, while its other extremity, E, remains invariably at the freezing point.

—and the extremes at the boiling and freezing temperatures.

By an experiment, of which I have already given an account to the class*, I found that when two equal bodies, A B, one hotter than the other, are isolated and placed opposite each other, the intensities of their radiations are such, that a third body, C, placed in the middle of the space that separates them, will acquire a temperature by the simultaneous action of these radiations, which will be an arithmetical mean between those of the two bodies A and B.

Then the middle particle would have the arith. mean temperature: A body midway between two others acquires the mean heat by radiation,

From the result of this experiment we have ground to conclude, that if the cylinder were composed of three particles of matter only, A, C, E, the particle C, which is in the middle of the cylinder, must necessarily have the arithmetical mean temperature between that of A and that of E, which are at the two extremities of the cylinder; that is to say, between 212° and 32° of Fahrenheit, which is 122°.

—or 122°.

Now let us interpose between the particles A, C, and E, two other particles B D, and see whether the introduction of these two particles will make any change in the temperature of the particle C that occupies the middle of the cylinder.

Suppose two more particles to be interposed.

If the particle B be placed in the middle of the space comprised between the extremity A of the cylinder and its middle, C, it ought to acquire a mean temperature between that of the extremity A of the cylinder, and that of the point C, namely that of 167°, the mean between 212° and 122°; and if the particle D be placed in the midst of the space comprised between the middle of the cylinder and its other extremity, E, this particle ought to acquire a mean temperature between that of the middle of the cylinder and that of its extremity E; it ought then to have the temperature of 77°.

These would each take the mean temperature between the middle particle and the nearest end particle;

From this new arrangement, the particle C, situate in the middle of the cylinder, will find for its neighbours on one side the

—and therefore they would not alter the temperature,

* See our Journal, IX. 193.

the particle B, at the temperature of 167°, and on the other the particle D, at that of 77°. The point in question is, whether the presence of these two particles will make any change in the temperature of the particle C, or not.

—of that middle particle.

In the first place it is evident, that if the calorific influences of the particle B on the particle C be as efficacious in heating it, as the frigorific influences of the particle D be in cooling it, the temperature of the particle C ought not to be changed. But experience has shewn, that, at equal distances and equal intervals of temperature, the calorific influences of hot bodies, and the frigorific influences of cold bodies, are exactly equal; and as the distance from B to C is equal to the distance from D to C, while the interval of temperature between B and C = 45°, is the same as that between D and C = 45°; it is evident that the temperature of the particle C, which is in the middle of the cylinder, can be no way affected by the introduction of the intermediate particles B and D.

And by the same reason it would not be changed by other particles interposed.

By the same way of reasoning may be proved, that the introduction of an indefinite number of intermediate particles would produce no change in the temperature of the middle of the axis of the cylinder, or in any part of it; and if the introduction of an indefinite number of intermediate particles make no change in the state of a thermometer placed in the middle of the axis of the cylinder, we may conclude that the thermometer would remain equally stationary, if the number of intermediate particles were increased till they had that proximity to each other which is necessary to constitute a solid body. If, instead of a single row of particles in a right line, there were a bundle composed of an indefinite number of such rows placed side by side, forming a solid cylinder, the temperature in the different parts of the line A E would remain the same.

But the temperature of a continued solid should decrease from one particle to another in arithmetical progression.

From this reasoning we may infer, that the temperatures of the different parts of the cylinder should decrease in arithmetical progression from one extremity of the cylinder to the other.

This is true only when the solid is remote from other bodies.

But it is evident, that this law of decrement of temperature could take place only in the single case of the surface of the cylinder being completely isolated, so as to be no way affected by the action of surrounding bodies, which is absolutely impossible.

Our experiments are always thus influenced.

The circumstances under which the experiments were made are very different from those here taken for granted. The bodies

bodies we subject to experiment are constantly surrounded on all sides by the air and other bodies which act on air instruments continually, and often in a very perceptible manner; and we can never hope to isolate a cylinder so completely that the apparent progress of heat in its interior shall perceptibly obey the law we have just discovered. In common cases it deviates widely from this law.

As the causes of this deviation are well known, we will see whether there be no means of appreciating their effects. Appreciation of the effect.

The surface of the cylinder being surrounded by the atmospheric air and other bodies, all which are of a known and sensibly constant temperature, we may determine the comparative effects of these bodies on the different parts of the surface of the cylinder. The atmosphere will affect the cylinder,

In those parts of the cylinder which are hotter than the air and other surrounding bodies, the surface of the cylinder will be cooled by the action of these bodies; but if one of the extremities of the cylinder be colder than the atmospheric air, those parts of the cylinder which are colder than the circumambient fluid will be *heated* by its influence and that of the surrounding bodies. —by cooling its hot part and heating its cold part.

We will begin with examining the case where the coldest extremity of the cylinder is at the same temperature as the surrounding air. Let us suppose then, that the experiment with boiling water at the one end and freezing at the other be made when the temperature of the air is at the freezing point, or 32° of Fahrenheit. If one extremity of the solid be at the temperature of the air, the other hotter,

In this case it is evident that the surface of the cylinder must every where be *cooled* by the influence of the surrounding atmosphere. The question then is to determine the comparative effects, or the relative quantities of refrigeration or loss of heat, that must take place *in the different parts of the cylinder:* and in the first place it is clear, that the hotter a given part of the cylinder is, the more heat it must lose in a given time, by the influence of the surrounding cold bodies; whence we may conclude, that the refrigeration of the surface of the cylinder by the influence of the air and other surrounding cold bodies must necessarily diminish from the extremity of the cylinder A, which is in contact with the hot water, to its extremity E, which is in contact with the cold. —the surface will be every where cooled, —but most so where the heat is greatest.

From

The change is in proportion to the difference of temperature.

From reaſoning which appears incontrovertible, and which the reſults of a great number of experiments appear to confirm, it has been concluded that the celerity with which a hot body placed in a cold medium is cooled, is always proportional to the difference between the temperature of the hot body and that of the medium. Conſidering this concluſion as eſtabliſhed, we may determine *a priori* what ought to be the gradation of temperatures in the interior of a given ſolid cylinder ſurrounded by air, one extremity of which is in contact with a conſiderable body of boiling water, while the other is ſimilarly in contact with cold.

We have ſeen that, if the ſurface of the cylinder were perfectly iſolated, the decreaſe of temperature from the hotteſt extremity of the cylinder A to its other extremity E, which is in contact with cold water would be *in arithmetical progreſſion*, and it has juſt been ſhewn, that the decreaſe muſt neceſſarily be accelerated by the action of the air and other ſurrounding cold bodies.

Whence the temperatures will be in geometrical progreſſion,

But the acceleration of the decreaſe of temperature in thoſe parts of the cylinder which are toward the cold extremity, depending on the action of the air and ſurrounding bodies, muſt be continually diminiſhing in proportion as the temperature of the ſurface of the cylinder approaches nearer and nearer that of the air; and hence we may conclude, that if a given number of points at equal diſtances from each other, be taken in the axis of the cylinder, the temperatures correſponding with theſe points will be *in geometrical progreſſion*.

—and may be repreſented by the logarithmic curve.

Figure conſtructed.

We may repreſent the progreſs of the decreaſe of temperature by *Fig.* 2. *Pl.* VII.

In a right line A E, repreſenting the axis of the cylinder, if we take the three points B, C and D, ſo that the diſtances A B, B C, C D, and D E ſhall be equal; and, erecting the perpendiculars A F, B G, C H, D I, E K, take A F = the temperature of the cylinder at its extremity A, B G = its temperature at the point B, and ſo of the reſt; the ordinates A F, B G, &c. will be in geometrical progreſſion, while their correſponding abſciſſes are in arithmetical progreſſion; conſequently the curve P Q, which touches the extremities of all theſe ordinates muſt neceſſarily be the *logarithmic* curve.

Compariſon of the theory with the experiment.

We will now ſee, whether the reſults of experiment agree with the theory here exhibited or not.

To

To form our judgment with ease, and as it were at a single glance, of the agreement of our theory with the results of the experiment, of which I gave an account at the beginning of this memoir, we have only to represent these results by a figure in the following manner.

On the horizontal line A E, *Fig.* 3. representing the axis of the cylinder employed in the experiment, we will take three points, B, C and D; one, C, in the middle of the axis, being the situation of the central thermometer, the other two, B and D, at the intermediate points which the other two thermometers occupied between the middle of the axis and its two extremities.

Construction of a figure exhibiting by a curve, and its ordinates the temperatures actually observed.

Erecting the perpendiculars A *f*, B *g*, C *h*, D *i*, and E *k*, on the points A, B, C, D and E; and taking the ordinate A *f*= 212, the temperature of boiling water; B *g*=162, the temperature indicated by the thermometer B; C *h*=132$\frac{1}{4}$, the temperature indicated by the thermometer C; D *i*=106$\frac{1}{2}$, the temperature given by the thermometer D; and lastly, E *k*= 32, the temperature of water mixed with pounded ice; a curve, P Q, passing through the points *f*, *g*, *h*, *i*, *k*, ought to be the *logarithmic*; that is, supposing the temperature of the surrounding air to be constantly at the temperature of melting ice during the experiment.

But the experiment in question was made when the temperature of the air was at 78° F. consequently, reckoning from a certain point, taken in the length of the cylinder, where the temperature was at 78°, to the extremity E, the influence of the surrounding air, instead of *cooling* the surface of the cylinder, *heated* it; and it is evident, that the curve P Q must necessarily in this case have a point of inflexion.

The curve has a point of contrary flexure.

In fact it appears on a simple inspection of the figure, that the curve P Q has a point of inflexion; but we see likewise, that this curve is not regular. That branch which is concave toward the axis of the cylinder is not similar to the adjoining portion of the curve, of equal length, which is convex toward that axis; as it ought to be according to our theory; and even the part of the curve which is convex toward the axis A E, differs sensibly from the logarithmic, particularly toward its extremity P.

It is likewise irregular.

It ought necessarily to differ from this curve, *as far as the divisions of our thermometers are defective*; but the deviation between

The deviation is too much to be owing to the defects of thermometers.

tween the ordinates A f and B g, indicated by the results of the experiment in question, appears to me much too considerable to be ascribed to the imperfection of our thermometers.

It differs greatly from the logarithmic.

To see how far the curve P Q differs from the logarithmic, we have only to draw a logarithmic curve R S through the points g and i, and we shall find, that the ordinates corresponding to the points

	A,	B,	C,	D,	E.
Instead of being	212°	162°	132°¼	106½	32°
Will be -	199.55	162	131	106½	86.35
Difference -	−2·45	0	−1¼	0	+54·35

Ascribed to water being a bad conductor of heat,

The very great difference that exists between the temperature of cold water, and that indicated by the results of the experiment for the extremity of the cylinder which was in contact with this water, led me to suspect, that it was owing to the quality possessed by water in common with other fluids, which renders it *a very bad conductor of heat*.

—and the currents in the cold water being inconsiderable.

If it be true, as I believe I have elsewhere proved, that there is no sensible communication of heat between the adjacent particles of a fluid, from one to another; and that heat is propagated through fluids only in consequence of a motion of their particles, resulting from a change in their specific gravity, occasioned by their being heated or cooled: as the specific gravity of water is very little altered by an inconsiderable change of temperature when this fluid is near the freezing point, it might have been foreseen, that a solid body a little heated, and plunged into cold water, would be very slowly cooled.

Experiment to prove this.

The result of the following experiment, which I made with a view to elucidate this point, will put the fact out of all doubt.

When the cold water was briskly stirred the thermometers were all greatly depressed.

The three thermometers being stationary, one, B, at 162°, the second, C, at 132¼°, and the third, D, at 106½°, the water in contact with one of the extremities of the cylinder being still boiling, while the water mixed with pounded ice, which was in contact with the other extremity, was constantly at the temperature of melting ice, I began to stir this mixture of ice and water pretty briskly with a little stick, and I continued to stir it uninterruptedly, and with the same velocity for two and twenty minutes.

I had scarcely begun this operation, when I had a proof, that

that my conjectures were well founded. The mercury in the three thermometers immediately began to descend, and did not stop till it had fallen very considerably.

The thermometer B fell from 162° to 152°; C from $132\frac{1}{4}$° to $111\frac{3}{4}$°; and D from $106\frac{1}{2}$° to $78\frac{1}{2}$°. Quantities of depression.

On comparing these numbers we find, that, in consequence of the agitation of the cold water for two and twenty minutes, the thermometer B fell 10° of Fahrenheit's scale, the thermometer C 21°, and the thermometer D 28°.

As soon as I had ceased to stir the cold water, the three thermometers began to rise, and at the end of a quarter of an hour they had all reached the points from which they set out at the beginning of this operation.

To facilitate the comparison of the results of these two experiments, one made with cold water at rest, the other with the same water in a state of constant agitation, I have represented them in *Fig*. 4. Diagram to represent these effects.

In the first place we shall learn several very interesting facts by simple inspection of this figure; we shall see, 1st. that the progress of refrigeration, or, to speak more properly, *the decrease of temperature*, was every where much more rapid, when the cold water in contact with the extremity of the cylinder E was agitated when it was at rest. Observations upon this experiment.

2dly. That the extremity of the cylinder in contact with this water was constantly near 30° colder in the first case than in the second.

3dly. We shall see, that the progress of refrigeration was every where, and in both the experiments, such nearly as our theory points out.

The decrease of temperature toward the middle of the cylinder was so regular, that it is more than probable the apparent irregularities toward the two extremities were occasioned solely by the difficulty which a body of water finds in communicating its mean temperature to a solid, with which it is in contact.

The boiling water being in continual motion owing to its ebullition, it had a great advantage over the cold water, which was at rest, in communicating its temperature to the extremity of the cylinder it touched; but I have found, notwithstanding this, that by agitating the boiling water strongly with a quill, and particularly when with the quill I made a rapid Agitation increased the effect of the boiling water likewise.

 friction

friction against the end of the cylinder immersed in the boiling water, I occasioned all the thermometers to rise several degrees.

The difference between the experiment and the theory confirm its truth;

It may perhaps be imagined, at first sight of the results of the experiment, that, as the three thermometers, which occupied the parts about the middle of the axis of the cylinder, did not indicate a decrease perfectly agreeing with the theory, the theory itself cannot be true: but a moment's reflection will show, that this inference would be too hasty, and that the difference between the theory and the results of our experiments, far from proving any thing adverse to the theory, serve on the contrary to render it more probable.

because the scales of our thermometers are defective.

The results of such experiments can never agree with the theory, except the divisions of our thermometers be perfectly accurate: but it is well known to every one, who has any knowledge of natural philosophy, that the divisions of our thermometers are defective.

To improve this instrument is an object of importance.

One of the objects I had in view in the experiments, of which I have just given an account to the class, and in several others, which I intend to make without delay, is to improve the division of the scale of the thermometer, in order to render this valuable instrument of greater utility in the delicate investigations of natural philosophy.

The air thermometer deserves to be attended to.

It appears certain, that the increase of the elasticity of air by heat is much more nearly proportionate to the increase of temperature, than the dilatation of mercury or any known fluid; consequently it is the air thermometer we ought to endeavour to improve, and which must ultimately afford us the most accurate measure of heat, that it is possible for us to procure.

Sect. IV. *The Heat produced in a Body by a given Quantity of solar Light is the same whether the Rays be denser or rarer, convergent, parallel, or divergent.*

Whether the quantity of heat generated by the solar rays be proportional to the light absorbed.

In all cases where the rays of the sun strike on the surface of an opake body without being reflected, heat is generated, and the temperature of the body is increased: but is the *quantity* of heat thus excited always in proportion to the quantity of light that has disappeared? This is a very interesting question, and has not hitherto found a decisive solution.

When

When we consider the prodigious intensity of the heat excited in the focus of a burning mirror or a lens, we are tempted to believe, that the concentration and condensation of the solar rays increase their power of exciting heat; but, if we examine the matter more closely, we are obliged to confess, that such an augmentation would be inexplicable. It would be equally so on both the hypotheses, which natural philosophers have formed of the nature of light; for, as it has been proved both by calculation and experiment, that two undulations in an elastic fluid may approach and even cross each other, without deranging either their respective directions or velocities, if light be analogous to sound, we do not see how the concentration or condensation of these undulations can increase their force of impulse: and if light be a real emanation, as its velocity is not altered, either by the change of direction it undergoes in passing through a lens, or by its reflection from the surface of a polished body, it seems to me, that the power of each of these particles to excite or impart heat, must necessarily be the same after refraction or reflection as before; and consequently, that the heat communicated or excited must be, in all cases, as the quantity of light absorbed.

It does not follow from reasoning that the *absolute* power of the rays to produce heat can be increased by condensing them.

I have just made some experiments, which appear to me to establish this fact beyond question.

Experimental investigation.

Having procured from the optician Lerebours two lenses perfectly equal, and of the same kind of glass, four inches in diameter, and of eleven and a half focus, I exposed them at the same time to the sun, side by side, about noon, when the sky was very clear; and by means of two thermometers, or reservoirs of heat, of a peculiar construction, I determined the relative quantities of heat, that were excited in given times by the solar rays at different distances from the foci of the lenses.

Two convex lenses perfectly similar were used,

The two reservoirs of heat are a sort of flat boxes of brass filled with water. Each of these reservoirs is three inches ten lines and a half in diameter, and six lines thick, well polished externally on all sides except one of its two flat faces, which was blackened by the smoke of a candle. On this face the solar rays were received in the experiments.

to throw the sun's light upon flat tin boxes containing water and blackened on their surfaces.

Each of these reservoirs of heat weighs when empty 6850 grains, *poids de marc*, (near a pound troy), and contains 1210 grains of water (about 2 oz. 2 dwts.

Taking

Taking the capacity of brass for heat to be to that of water as 0,11 to 1, it appears, that the capacity of the metallic box, weighing 6850 grains, is equal to the capacity of 622 grains of water; and adding this quantity of water to that contained in the box, we shall have the capacity of the reservoir prepared for the experiments equal to that of 1932 grains of water.

The temperature of the water in each was shewn by a thermometer.

Each reservoir is kept in its place by a cylinder of dry wood, one of the extremities of the cylinder being fixed in a socket in the center of the interior face of the reservoir; and each reservoir has a little neck, through which it is filled with water, and which after receives the bulb of a cylindrical thermometer, that reaches completely across the inside of the box in the direction of its diameter.

The two reservoirs of heat, with their two lenses, are firmly fixed in an open frame, which being moveable in all directions by means of a pivot and a hinge, the apparatus is easily directed toward the sun, and made to follow its motion regularly, so as to keep the solar spectra constantly in the centers of the blackened faces of the reservoirs.

Light admitted through equal apertures.

In order that the quantities of light passing through the two lenses should be perfectly equal, a circular plate of well polished brass, in the centre of which is a circular hole three inches and a half in diameter, is placed immediately before each of the lenses.

When the reservoirs of heat are placed at different distances from the focuses of their respective lenses, the diameters of the solar spectra, which are formed on the blackened faces of the reservoirs, are necessarily different; and as the quantities of light are equal, its density at the surface of each reservoir is inversely as the square of the diameter of the spectrum formed on that surface.

Experiment I.

Experiment. With equal apertures the solar spots from the lenses were of 6 and of 24 lines diam.

In this experiment the reservoir A was placed so near the focus of the lens, between the lens and the focus, that the diameter of the solar spectrum falling on it was only $\frac{1}{2}$ an inch, or 6 lines, while the reservoir B was advanced so far before the focus, that the spectrum was two inches in diameter, or 24 lines.

As

As the quantities of light falling on both were equal, the density of the light at the surface of the reservoir A was to the density of that at the surface of the reservoir B, as the square of 24 to the square of 6, or as 16 to 1.

The densities of these equal quantities of light were therefore as 16 to 1;

I imagined, that, if the quantity of heat, which a given quantity of light is capable of exciting, depended any way on its density, as the densities were so different in this experiment, I could not fail to discover the fact by the difference of time, which it would require to raise the two thermometers the same number of degrees.

Having continued the experiment more than an hour, on a very fine day, when the sun was near the meridian and shone extremely bright, I did not find, that one of the reservoirs was heated perceptibly quicker than the other.

but both the vessels were heated in equal times.

Experiment II.

I placed the reservoir of heat A still nearer the focus of the lens, in a situation where the solar spectrum was only $4\frac{1}{4}$ lines in diameter, and where blackened paper caught fire in two or three seconds; and I removed the reservoir B still farther from the focus, advancing it forward till the diameter of the spectrum was two inches three lines.

Experiment wherein the diameters of the spots were as $4\frac{1}{4}$ to 27.

The densities of the light at the surfaces of the reservoirs in this experiment were as 32 to 1.

The densities of the light were as 32 to 1.

The temperature of the reservoirs, as well as that of the atmosphere, at the beginning of the experiment, was 54° F. = $9^\circ \frac{7}{9}$ R.

The reservoir A, after having been exposed to the action of very intense light near the focus of the lens for twenty-four minutes forty seconds, was raised to the temperature of 80° F. = $21^\circ \frac{1}{3}$ R.

The densest light afforded rather less heat.

The reservoir B, which was much farther from the focus of its lens, was raised to the same temperature, 80° F. a little more quickly, or in twenty-three minutes forty seconds.

To raise the temperature of the reservoir A to 100° F. = $30^\circ \frac{2}{9}$ R. it was necessary to continue the experiment for one hour fifteen minutes ten seconds, reckoning from the commencement of it; but the reservoir B reached the same temperature in one hour twelve minutes ten seconds.

The progress of this experiment from the beginning to the end is exhibited in the following table:

Increases

The general results tabulated.

Increases of Temperature.	Time taken By A.	Time taken By B.
From 54° to 80° F.	21′ 40″	23′ 40″
80 85	7 45	7 30
85 90	9 55	9 0
90 95	13 30	13 0
95 100	19 20	19 0
54 100	75 10	72 10

Time of the experiment.

This experiment was begun at 7 minutes 30 seconds after 11, and finished at 22 minutes 40 seconds after 12, the sky being perfectly clear during the time.

Hence light does not give more heat absolutely by being condensed.

On comparing all the results of this experiment, we see, that the reservoir A, which was placed very near the focus, was more slowly heated than the reservoir B, which was at a considerable distance from it *. The differences of time however taken to heat them an equal number of degrees were very trifling, and I think may be easily explained, without supposing the condensation of light to increase (*qu.* diminish?) its faculty of exciting heat.

The rays were convergent in the preceding experiments.

In both the preceding experiments the solar rays striking on the reservoirs of heat were *convergent*, and they were even equally so on both sides. To determine whether *parallel* rays have the same power of exciting heat as convergent rays, I made the following experiment.

Experiment III.

When one vessel was exposed to the parallel rays of the sun without interception,

Having removed the lens from before the reservoir B, I suffered the direct rays of the sun to fall on the blackened face of the reservoir, through the circular hole three inches and half in diameter in the round brass plate, which had been constantly placed before that lens in the preceding experiments.

The reservoir A was placed behind its lens as in the former experiments, and at the place where the solar spectrum had six lines diameter.

* Did not the elevated temperature of the smaller surface sustain its power of absorbing heat, conformably to the known laws of heated bodies?—N.

Having

Having expofed this apparatus to the fun, I found, that the refervoir B, on which the direct rays fell, was heated fenfibly quicker than the refervoir A, which was expofed to the action of the concentrated rays near the focus of the lens.

It was heated more quickly than the other by a fpectrum of 6 lines or one-feventh part diam.

The temperature of the apparatus and of the atmofphere at the beginning of the experiment being 53° F. = 9° $\frac{1}{3}$ R. the refervoir A required twenty-three minutes thirty feconds to raife it to the temperature of 80° F. = 21° $\frac{2}{9}$ R; but the refervoir B, which was expofed to the direct rays of the fun, acquired the fame temperature in eighteen minutes thirty feconds.

To reach the temperature of 100° F. = 30° $\frac{2}{9}$ R. took the refervoir A one hour and three minutes, but the refervoir B forty-feven minutes fifteen feconds only.

The following table will fhow the progrefs of this experiment from the beginning to the end.

General refults.

Increafes of Temperature.		Time taken By A.		Time taken By B.	
From 53°	to 65° F.	8′	26″	7′	0″
65	70	4	10	3	15
70	75	5	10	3	45
75	80	5	40	4	30
80	85	7	0	4	45
85	90	7	30	5	45
90	95	10	30	8	0
95	100	13	10	10	15
100	105	20	0	14	45
53	105	81	36	62	30

As a confiderable part of the light that fell on the lens before the refervoir A, was loft in paffing through it, it is evident, that the quantity received by this refervoir was lefs than that received by the refervoir B, which was expofed to the direct rays of the fun; and we have feen, that the latter was heated more rapidly than the former.

This difference afcribed to light being loft in paffing through the lens.

As we know not exactly how much light was loft in paffing through the lens, we cannot determine from the refults of this experiment, whether convergent rays be more or lefs efficacious in exciting heat than parallel rays; but the difference in the times

This experiment is not decifive: but

times of heating was not greater, as it appears to me, than we might have expected to find it, supposing it to be occasioned solely by the difference between the quantities of light acting on the reservoirs.

The result of the following experiment will establish this point beyond doubt.

Experiment IV.

Exp. 4 was made with equal apertures and spectra; but the one being *within* the focus was formed by convergent rays, and the other *without* by divergent rays.

Having replaced the lens belonging to the reservoir B, I adjusted this reservoir to such a distance between the lens and its focus, that the solar spectrum was one inch in diameter; and I placed the reservoir A at the same distance beyonds its focus.

As the quantities of light directed toward both were equal; and the diameters of the spectra, consequently the densities of the light that formed them, were also equal; there could be no difference between the results of the experiments with the two reservoirs, except what was occasioned by the difference in the *direction* of the rays that formed the spectra. On one hand these rays were *convergent*, and on the other *divergent*; and I had inferred, that if parallel rays were in reality less efficacious in exciting heat than convergent rays, as some philosophers have supposed, *divergent* rays must be still less efficacious than parallel rays, and consequently much less than convergent rays.

No sensible difference occurred.

Having made the experiment with all possible care, I found no sensible difference between the quantities of heat excited in a given time by divergent and convergent rays.

The following are the particulars of the progress and results of this experiment:

General results of this last experiment.

Increases of Heat.		Time taken			
		By A, with divergent Rays.		By B, with convergent Rays.	
From 60°	to 65° F.	4′	50″	4′	50″
65	70	4	55	5	0
70	75	5	27	5	25
75	80	6	13	6	15
60	80	21	25	21	30

From

From the results of all the experiments, of which I have just given an account to the class, we may conclude, that the quantity of heat excited or communicated by the solar rays is always, and under all circumstances, as the quantity of light that disappears.

Conclusion. The quantity of heat is always as the light absorbed.

III.

Observations on blasting Rocks; with an Account of an Improvement, whereby the Danger of accidental Explosion is in a great Measure obviated. By Mr. WILLIAM CLOSE. *From the Author.*

To Mr. NICHOLSON.

SIR, *Dalton, Oct.* 14, 1805.

THE method of confining the force of gunpowder by a column of sand in blasting rocks, has been several years used in this part of Furness: At one time it was a very favourite practice; but at present, from the prejudices or indifference of workmen, or on account of the little danger attendant on working lime-stone in the common manner, it is less in repute.

Practice of blasting with sand in Furness,

About two years ago, supposing this method not to be generally known, I drew up a short account of it, and should have sent it to the Philosophical Journal, had it not been connected with other miscellaneous matter, which I had given to Mr. G. Ashburner, the printer and proprietor of a new edition of West's Antiquities of Furness, in which work the process is described and recommended *.

noticed by the author elsewhere.

Though

* The passage alluded to is as follows, p. 393. " In breaking up the loose rocks upon Baycliff Haggs, after the enclosure of that common, a method of employing sea-sand, for the purpose of confining the force of gunpowder in blasting, was used, which does not appear to be generally known, though it was undoubtedly in use in other parts before it was adopted in Furness. The method is briefly this: After the excavation is made in the usual manner with a borer, the charge of powder is poured in; and a priming-straw of a proper length, filled with powder, is placed in the hole, having one of its sides near the lower end so cut or thinned, that the

Improvement by Mr. Fisher announced.

Though this method is undoubtedly worthy of much attention, and may often be employed with advantage; yet, when a strong charge is required, the common mode of stemming must be frequently adopted: And as the danger in blasting some kinds of rocks in this manner is very considerable, I am happy to notice an easy method of obviating one principal cause of accidental explosion, which was communicated to me in conversation, a few days ago, by Mr. Thomas Fisher, a respectable slate merchant in this town, who assures me it is infallible,

Causes of accidental explosion. The principal is from the friction of the iron pricker in drawing.

The principal danger attendant on blasting, does not consist in stemming upon the charge of powder, but in the subsequent operation of drawing the iron rod, called the pricker, which makes the channel for the priming-straw. For although the collision of the first fragments of stemming sometimes produces an explosion, yet this may be prevented by previously ramming a thick cap of paper, &c. upon the powder, by beating lightly upon the first pieces of stone that are thrown into the hole; or by using those materials for stemming which are least liable to give fire, such as rotten stone, pieces of broken pots, or burnt clay. The pricker being hard pressed against the rock, and in close contact with the stemming, cannot be drawn out by hand, but must be struck out by the hammer, a strong piece of iron called a jumper being first placed in an eye or loop in the highest part of the rod, to receive the blows which are given in a proper direction to bring it out of its place. Now it frequently happens, that the friction of the lowest part of the pricker against the rock fires the powder at the first or second blow. When the explosion happens at the commencement of stemming, the workman generally sustains only a partial injury; but when in this part of the operation, when the powder exerts its whole force, and

the charge may partially communicate with the small ascending column contained in the straw. After this, the remainder of the excavation is filled, by pouring in dry sea-sand; and the explosion is given, by firing the priming-straw in any of the various ways which are in common use.

"This method has been found to be equally effectual as stemming with any of the common materials; and where it can be used is certainly preferable: it is safer, simpler, and more expeditious."

disperses

disperses pieces of the shattered rock in various directions, his life is in the utmost danger, and his situation is truly terrible to contemplate.

Mr. Fisher's improvement is to obviate this danger; and consists in the use of a copper rod, or pricker, for making the hole that receives the priming-straw, instead of one of iron, which before was every where employed in this part of the kingdom.

Mr. F.'s expedient is a pricker made of copper, which is not liable to fire the powder.

In our conversation Mr. F. observed, that some years ago, three explosions happened on drawing the pricker, in the course of a fortnight, at his quarry in Kirkby Ireleth, and that one man being killed and two wounded, several of the workmen were so intimidated, that they resolved to abandon a place which they considered as destined to daily misfortunes. It therefore became highly requisite, on several accounts, to attempt some innovation for the security and encouragement of the workmen.

In meditating on the cause of these accidents, it appeared most rational to attribute them to the iron pricker giving fire by its friction against the rock, which was a hard blue rag, or whinstone: and from this view of the cause it was inferred, that safety would accrue from the use of prickers constructed of those metals which are least disposed to give fire with stone. Mr. Fisher, therefore, determined to make trial of copper, and having procured some implements of this kind, found them to answer the purpose completely. It is now upwards of three years since this improvement was adopted, and as no explosion has happened at the end of stemming in that period, at an extensive work where accidents were frequent before, Mr. F. considers the means as almost infallible; and is happy to think that many sad misfortunes have been thereby prevented.

Ample experience has proved its utility.

There are eleven slate quarries in Kirkby Ireleth, at several of which copper rods are now used; but at others they are not. At one of these a fatal accident happened a few months ago, from an explosion upon drawing a rod of iron.

Prickers, such as used by Mr. Fisher, are easily constructed: A piece of copper being forged to the proper length, shape, and thickness for the body of the tool, is rivetted to an iron head or loop similar to that of the common pricker. These implements, when carefully used, are nearly as durable as those of iron.

Advantage of strong charges in firm rock.

Sand has not hitherto been used in blasting at the slate quarries in Kirkby Ireleth. The masters do not think it would succeed well in their work. I have frequently seen Mr. Fisher use it in limestone rock near this town: He says it answers the best in deep holes, but thinks that sand is more liable to be blown out than stemming. He also considers it as the most advantageous method of working, in driving levels, and blasting in firm rock, to use strong charges of powder, that the stone may be sufficiently broken by the explosion to be removed without much assistance from the hammer, the pick, or the lever: For thus the expedition of the work amply compensates for the small addition which is requisite to a common charge of powder.

I am, Sir,

Your's respectfully,

WILLIAM CLOSE.

IV.

Description of a portable Steam-engine, invented by Mr. Samuel Clegg*, *David Street, Manchester. Communicated by Mr.* Dalton, *Lecturer at the Royal Institution, &c.*

Description of a steam-engine.

THIS engine is worked by four copper valves in the usual manner, but the mechanism for lifting them is very different from any hitherto made: there are no levers employed for opening the valves, and there is no hand gear. The steam and exhaustion valves are on the same horizontal plane; those which are vertical to each other are not like those hitherto used, both exposed to the steam or both to a vacuum; but by a simple contrivance in the construction of the nozzles, the one is exposed to the steam while the other has a communication with the condensing vessel. From what has been said it may easily be perceived, if the two valves be connected together by a straight rod, that when this rod is lifted, the pressure is given to the piston, and the machine is put into motion; and if the other two valves be connected in the same

* Late apprentice to Messrs. Boulton and Watts, of Birmingham.

manner

manner and lifted at an appointed time, the engine is kept in motion. The outside appearance of these nozzles may be seen at *Fig.* 1, *c c*, (*Plate* IX.) The rods which come out of the bottom of the nozzles are kept tight by vertical stuffing-boxes, the whole of which is hid in the drawing by the frame.

Description of a steam-engine.

The next is a new contrivance for producing a rotative motion from a reciprocating one, which not only simplifies the machine very much, but exceeds the power of the common crank by nearly one-third, in consequence of its acting always perpendicular to the radius of the wheel, which is done by a rack and wheel, as represented by *Fig.* 2 and 3; and as this plan of connection distributes the power uniformly, of course a much lighter fly-wheel is required, which diminishes friction, &c.

Explanation of the Plate.

Fig. 1. is a representation of the engine: one of the corner columns A A, which supports the frame, serves likewise for an eduction-pipe and condensing-vessel: the air-pump E is joined to the condensing vessel by the pipe D; *e* is the piston-rod, and though it works out at the bottom of the cylinder, it is as easily kept tight as if it worked out at the top; *b* is a similar rod which keeps the rack perpendicular; *a a* are the two radius bars on which the brasses are fixed that support the shaft; by this contrivance the wheel C easily moves from one side to the other of the rack F.

Fig. 2. is a view of the rack on a larger scale, where C represents the wheel and D the shaft; E E, a sliding-bar, on which is fixed the small roller *o*, serving as a connecting link to keep the wheel C always in gear; for, when the wheel is in gear on the opposite side of the rack, the roller *o* is on the other side of the plate *a a*; but it will perhaps be more clearly understood by the plan, *Fig.* 3. where the letters represent the same movement as in the elevation, *Fig.* 2: This description may be easily understood by those who already possess a little knowledge of a steam-engine.

Manchester, Oct. 5, 1805.

Letter

V.

Letter from Mr. J. C. HORNBLOWER, *describing the framed Work by which the Roof of Clapham Church was raised to its original Situation, without disturbing the Interior of the Building, &c.*

To Mr. NICHOLSON.

DEAR SIR,

Framed truss by which the roof of Clapham church was raised, &c.

IT will be a pleasure to you I know to record the productions of genius or fancy in your valuable Work, and therefore I have no hesitation in presenting the inclosed for that purpose.

It is the invention of Mr. Watkin Bloore, one of the partners of Fothergal and Co. carpenters at Clapham, and was invented to raise the sunk roof of Clapham church; and its application to the purpose intended, exhibits at once the *means* and the *end* that was to be accomplished; as by it the roof was *raised* and *secured* in the same process, without incommoding the building with shoors and scaffolds, which must have occasioned considerable damage to the furniture of the church.

The shaded part of the drawing, *Plate* X. shews the truss, and the lines behind it the construction of the roof. The middle piece in the truss marked A, is joggled into the king-post of the roof, and the two screws put into action raise it up, and with it the whole of the middle or sunk part of the roof, all which is easily comprehended by the drawing.

The drawing, *Fig.* 3, shews an improved mode of constructing the truss, by the riders A A A A being framed over the principals B B, by which the raising screws are more firmly supported in elevating the queen-posts C C in the roof.

This must be a valuable experiment in the art of carpentry, which, considering how little science of it falls to the lot of its possessors, cannot be too much regarded.

I am, Dear Sir,

Your very obedient servant,

J. C. HORNBLOWER.

Remarks

VI.

Experiments on draining Land, by JOHN CHRISTIAN CURWEN, *Esq. M. P. of Workington-Hall, in Cumberland, with an Engraving* *.

DEAR SIR,

MUCH having been said, in the public Papers, relative to draining, on the improved method of Mr. Elkington, I beg leave to offer you some observations respecting it, which have fallen under my notice, and which tend to prove it can be applied, with success only, in such parts of the kingdom, as have few, if any, interruption of the strata. In order to make myself intelligible, it may not be improper to explain what is meant by interruptions of the strata, or dykes and fissures, as they are denominated in mining countries. They are produced by the fracture or disunion of the strata, and consist most commonly of the broken fragments of each superior strata; and towards the surface are of sand, gravel, and stones, which seldom or never fail of affording considerable quantities of water. These dykes may be approached within a few feet, and afford no water, as will be seen in two instances in the plan sent you. No. 3 is a main drain, four feet deep, which passed within a few yards of A, an extreme wet place, and did not affect it. The person employed, supposed the water to be below him, and brought in a lower level No. 1, which likewise failed. No. 2 was then made still lower, but with no better success than No. 3, though with more advantage of level. As soon as it crossed the dyke, I C, but before the level was brought up, not being deeper than the main drain, it got a considerable feeder. This proved that an interruption in the strata prevented the water flowing into a drain, which was of a depth otherwise to have drawn it. Another example occurs in the same field, at letter B; which is a sunk fence, four feet below the surface of the adjoining field, which was extremely wet within a few yards of the sunk fence.

Mr. Elkington's method of draining is applicable only where the strata are little interrupted.

Dykes and fissures.

These interruptions prevent the draining off of the water.

* From the twenty-second volume of the Transactions of the Society of Arts; who awarded the gold medal to the author. The plan he refers to is at their house.

Experiments of drainage.

fence. A lower level was fuppofed neceffary to drain this water, and it was obtained at the dotted line. No water of any confequence was got, till it was within a few yards of the funk fence, when a prodigious feeder was cut, and the head of the drain was not fo deep at the time as the funk fence. Many inftances to the fame effect might be produced. In finking fhafts in places much troubled with water, it is endeavoured, if circumftances will permit, to get near a dyke, which ferves as a barrier to the water; and if, in finking, the dyke be not croffed, the water is kept clear off; but if otherwife, the water would be got at any depth, though not in fuch quantities as when near the furface. The fpot of ground, to which I have alluded, has above a dozen dykes, which may be traced from the out-burfts of water. They run in a direction of fouth to north-weft. I have made my drains eaft and weft. In one or two places, I was obliged to run a drain fouth. This proceeds from an arm running from the dyke: but this feldom extends to any diftance, and they gradually decreafe till they end; and they rather make an interruption than a breakage of the ftrata, as the ftrata is the fame on each fide of it. In fuch a country, Mr. Elkington could draw no more water than what lay in the uninterrupted ftrata between any two of thefe dykes. The method of making the drain is explained by the engraving. I had twenty years ago drained this ground with ftone drains, from 20 inches to two feet; but their direction having been moftly from north to fouth, and not fufficiently deep, I had got little more than the day water. The feeder which I have now got, might be made applicable to many purpofes. The drains are from two feet to nearly five feet deep. I have made 6000 yards in the laft twelve months; the cutting from 14d. to 18d. per rod, filling 8d. ten and a half fingle cart-loads of ftones, at 9d. each, making the coft 10s. per rod. The expence appears great; but fewer drains are required, and the work is effectually done. By reference to the plan, it will be feen that the direction of the drains not being able to draw the upper water, I was obliged to extend them. I would advife beginning at the higheft level; for frequently that clears the whole, unlefs fome dykes intervene in a contrary direction. I believe that all fprings and outburfts of water proceed from dykes. The extent of thefe is various. Some may be traced for many miles, and their effects

The drains muft be made to crofs the dykes.

Defcription of the drains by reference to the drawing.

Springs of water proceed from dykes.

feen

ſeen from the water that appears on the ſurface. Their origin is ſcarcely perceptible, and they thicken to many yards as they are approached. The ſtrata on both ſides have a more rapid riſe or dip, and are of a cloſer and harder texture. If theſe obſervations appear to you worthy of attention, you may make what uſe you think proper of them. I by no means wiſh to detract from Mr. Elkington's merit; but it is not generally applicable; and in counties where the ſtrata are much broken, Mr. Elkington's plan will be found to fail.

I am, Dear Sir,

Your obedient ſervant,

J. C. CURWEN.

Feb. 3, 1801.

Mr. Charles Taylor.

P. S. The higheſt drain is 120 feet above the level of No. 3.

A certificate from Mr. William Hoodleſs, farming agent, accompanied this letter, ſtating that upwards of ſix thouſand yards of drains had been cut, and completely filled, on the farm of John Chriſtian Curwen, Eſq.; that the firſt drains made according to that plan were done three years ago; and that they ſtand completely, and anſwer an admirable purpoſe.

Reference to Fig. 5, Plate XI. *of the Manner in which Mr.* CURWEN's *Drains are made.*

The loweſt part of the drain below E E is twelve inches wide.

E E 4 4 are the two ſide-ſtones of the drain, nearly four inches thick and nine inches high.

F 9 is the aperture for the water, nine inches high.

D, the flag or thin ſtone over the aperture, and which covers the ſide-ſtones of the drain.

C C, the body of the drain, filled with looſe ſtones till within nine inches of the ſurface.

B 9, the top of the drain, twenty-two inches wide and nine inches deep, filled with graſs ſod and ſoil.

VII.

Remarks on a Letter of Mr. DALTON, *concerning the Maximum Denſity of Water; with an Account of two Experiments of Dr.* HOPE, *tending to ſhew that it takes place at a Temperature above the freezing Point. In a Letter from* T. I. B.

To Mr. NICHOLSON.

SIR,

Reference to Mr. Dalton's letter.

IN No. 45 of your Journal, page 28, Mr. Dalton has publiſhed ſome remarks upon Count Rumford's experiments, relating to the maximum denſity of water, where he explains the riſing of the thermometer in the cup, by obſerving that it acquired heat by the proper conducting power of water. This, I ſhould think, is by no means probable; for the conducting power of water is not ſufficient to produce ſuch a rapid effect.

The circumſtances of the two thermometers by the ſide o the ball and cup, in the Count's two firſt experiments, I think are perfectly conſiſtent with his principle: the cup, very probably, did overflow, which might have been aſcertained by a thermometer placed below.

In the concluſion of this letter Mr. Dalton expreſſes a wiſh that Count R. or ſome one in poſſeſſion of a ſimilar apparatus, would repeat the Count's firſt experiment, with this difference, that the maſs of water ſhould be at 40° and the ball at 32°, in which caſe, he ſays, the thermometer would not be at all affected on the Count's principle; neither would it be affected (on Mr. D.'s principle) if the tenacity of the water counteracts the force of deſcent: and what concluſion could be drawn from ſuch a variation of the experiment?

Experiments of Dr. Hope.

The following experiment was made by Dr. Hope, profeſſor of chemiſtry in Edinburgh, to aſcertain the point at which water has the greateſt denſity,—and it appears to me to be perfectly deciſive.

He filled a jar with ice-cold water, and expoſed it to the air of a room at 52°: he ſuſpended in it two very delicate thermometers, one at half an inch from the bottom, and the other at the ſame diſtance from the ſurface of the water: the thermometer neareſt the bottom was firſt affected, and con-

 tinued

tinued to riſe till it reached the temperature of 40°, when it became ſtationary: the thermometer at the ſurface roſe more ſlowly, but did not ſtop till it acquired the temperature of the room (52°).

Dr. Hope, to render this point ſtill clearer, expoſed water at 52° to the air of a room 32°; the reſult correſponded perfectly with the former experiment.

If you ſee no objection to the publication of this letter, by inſerting it in your Journal, you will oblige *,

Sir, your's,

T. I. B.

Edinburgh, Oct. 10, 1805.

VIII.

Obſervations and Conjectures relative to the ſuppoſed Welch Indians in the weſtern Parts of North America. Republiſhed from the "Kentucky Palladium," with additional Remarks and Conjectures, by the Editor of the Philadelphia Medical and Phyſical Journal †.

SIR,

NO circumſtance relating to the hiſtory of the Weſtern Country, probably has excited, at different times, more general attention and anxious curioſity than the opinion, that a nation of white men, ſpeaking the Welch language, reſide high up on the Miſſouri. By ſome the idea is treated as nothing but the ſuggeſtion of bold impoſture and eaſy credulity; whilſt others regard it as a fact fully authenticated by Indian teſtimony and the report of various travellers worthy of credit.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

The fact is accounted for, they ſay, by recurring to a paſſage in the hiſtory of Great Britain, which relates, that ſeveral years after the diſcovery of America by Chriſtopher Columbus, a certain Welch prince embarked from his native country with a large party of emigrants; that after ſome time, a veſſel

* A fuller account of the late experiments of Dr. Hope will be inſerted when the Edinburgh Tranſactions appear.

† Extracted from that Work, Vol. II. Part I.

or

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or two came back with the account that they had discovered a country far to the westward, and that they set sail again with a fresh reinforcement, and never returned again any more.

The country which these adventurers discovered, it has been supposed, was the continent of North America; and it has been conjectured that they landed on the continent, somewhere in the Gulf of Mexico, and from thence proceeded northward, till they got out of the reach of the hostile natives, and seated themselves in the upper country of Missouri.

Many accounts accordingly have been published, within the last thirty years, of persons who, in consequence either by accident or the ardour of curiosity, have made themselves acquainted with a nation of men on the Missouri, possessing the complexion of Europeans and the language of Welchmen.

Could the fact be well established, it would afford perhaps the most satisfactory solution of the difficulty occasioned by a view of the various ancient fortifications with which the Ohio country abounds, of any that has ever been offered. Those fortifications were evidently never made by the Indians. The Indian art of war presents nothing of the kind. The probability too is that the persons who constructed them were, *at that time*, acquainted with the use of iron: The situation of these fortifications, which are uniformly in the most fertile land of the country, indicates, that those who made them were an agricultural people; and the remarkable care and skill with which they were executed, affords traits of the genius of a people, who relied more on their military skill than on their numbers. The growth of the trees upon them is very compatible with the idea that it is not more than three hundred years ago that they were abandoned.

These hints however are thrown out rather to excite enquiry, than by way of advancing any decided opinion on the subject. Having never met with any of the persons who had seen these white Americans, nor even received their testimony near the source, I have always entertained considerable doubts about the fact. Last evening, however, Mr. John Childs, of Jessamine County, a gentleman with whom I have been long acquainted, and who is well known to be a man of veracity, communicated a relation to me, which at all events appears to merit serious attention.

After

After he had related it in converſation, I requeſted him to repeat it, and committed it to writing. It has certainly ſome internal marks of authenticity. The country which is deſcribed was altogether unknown in Virginia when the relation was given, and probably very little known to the Shawnees Indians. Yet the account of it agrees very remarkably with later diſcoveries. On the other hand, the ſtory of the large animal, though by no means incredible, has ſomething of the air of fable, and it does not ſatisfactorily appear how the long period which the party were abſent was ſpent; though Indians are, however, ſo much accuſtomed to loiter away their time, that many weeks, and even months, may probably have been ſpent in indolent repoſe.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

Without detaining you any more with preliminary remarks, I will proceed to the narration, as I received it from Mr. Childs.

Maurice Griffith, a native of Wales, which country he left when he was about ſixteen years of age, was taken a priſoner by a party of Shawnees Indians, about forty years ago, near Voſſes Fort, on the head of Roanoke river in Virginia, and carried to the Shawnees nation. Having ſtaid there about two years and a half, he found that five young men of the tribe had a deſire of attempting to explore the ſources of the Miſſouri. He prevailed upon them to admit him as one of the party. They ſet out with ſix good rifles and with ſix pounds of powder a-piece, of which they were, of courſe, very careful.

On reaching the mouth of the Miſſouri, they were ſtruck with the extraordinary appearance occaſioned by the intermixture of the muddy waters of the Miſſouri and the clear tranſparent element of the Miſſiſſippi. They ſtaid two or three days amuſing themſelves with the view of this novel ſight: they then determined on the courſe which they ſhould purſue, which happened to be ſo nearly in the courſe of the river, that they frequently came within ſight of it as they proceeded on their journey.

After travelling about thirty days through pretty farming wood land, they came into fine open prairies, on which nothing grew but long luxuriant graſs. There was a ſucceſſion of theſe varying in ſize, ſome being eight or ten miles acroſs but one of them ſo long that it occupied three days to travel through

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through it. In paſſing through this large prairie, they were much diſtreſſed for water and proviſions, for they ſaw neither beaſt nor bird; and, though there was an abundance of ſalt ſprings, freſh water was very ſcarce. In one of theſe prairies the ſalt ſprings ran into ſmall ponds, in which, as the weather was hot, the water had ſunk and left the edges of the ponds ſo covered with ſalt, that they fully ſupplied themſelves with that article, and might eaſily have collected buſhels of it. As they were travelling through the prairies they had likewiſe the good fortune to kill an animal, which was nine or ten feet high, and a bulk proportioned to its heighth. They had ſeen two of the ſame ſpecies before, and they ſaw four of them afterwards. They were ſwift-footed, and they had neither tuſks nor horns. After having paſſed through the long prairie, they made it a rule never to enter on one which they could not ſee acroſs, till they had ſupplied themſelves with a ſufficiency of jerked veniſon to laſt ſeveral days.

After having travelled a conſiderable time through the prairies, they came to very extenſive lead mines, where they melted the ore, and furniſhed themſelves with what lead they wanted. They afterwards came to two copper mines, one of which was three miles through; and in ſeveral places they met with rocks of copper ore as large as houſes.

When about fifteen days journey from the ſecond copper-mine, they came in ſight of white mountains, which, though it was in the heat of ſummer, appeared to them to be covered with ſnow. The ſight naturally excited conſiderable aſtoniſhment; but, on their approaching the mountains, they diſcovered that, inſtead of ſnow, they were covered with immenſe bodies of white ſand.

They had in the mean time paſſed through about ten nations of Indians, from whom they received very friendly treatment. It was the practice of the party to exerciſe the office of ſpokeſman in rotation; and when the language of any nation through which they paſſed was unknown to them, it was the duty of the ſpokeſman, a duty in which the others never interfered, to convey their meaning by appropriate ſigns.

The labour of travelling through the deep ſands of the mountains was exceſſive; but at length they relieved themſelves of this difficulty, by following the courſe of a ſhallow river, the bottom of which being level, they made their way to the top of the mountains with tolerable convenience.

After

After paſſing the mountains they entered a fine fertile tract of land, which having travelled through for ſeveral days, they accidentally met with three white men in the Indian dreſs. Griffith immediately underſtood their language, as it was pure Welch, though they occaſionally made uſe of a few words with which he was not acquainted. However, as it happened to be the turn of one of his Shawnees companions to act as ſpokeſman or interpreter, he preſerved a profound ſilence, and never gave them any intimation that he underſtood the language of their new companions.

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After proceeding with them four or five days journey, they came to the village of theſe white men, where they found that the whole nation was of the ſame colour, having all the European complexion. The three men took them through their villages for about the ſpace of fifteen miles, when they came to the council-houſe, at which an aſſembly of the king and chief men of the nation was immediately held. The council laſted three days, and as the ſtrangers were not ſuppoſed to be acquainted with their language, they were ſuffered to be preſent at their deliberations.

The great queſtion before the council was, what conduct ſhould be obſerved towards the ſtrangers. From their fire-arms, their knives, and their tomahawks, it was concluded that they were a warlike people. It was conceived, that they were ſent to look out for a country for their nation; that if they were ſuffered to return, they might expect a body of powerful invaders; but that if theſe ſix men were put to death, nothing would be known of their country, and they would ſtill enjoy their poſſeſſions in ſecurity. It was finally determined that they ſhould be put to death.

Griffith then thought it was time for him to ſpeak. He addreſſed the council in the Welſh language. He informed them, that they had not been ſent by any nation; that they were actuated merely by private curioſity, they had no hoſtile intentions; that it was their wiſh to trace the Miſſouri to its ſource; and that they ſhould return to their country ſatisfied with the diſcoveries they had made, without any wiſh to diſturb the repoſe of their new acquaintances.

An inſtant aſtoniſhment glowed in the countenances, not only of the council, but of his Shawnees companions, who clearly ſaw that he was underſtood by the people of the country. Full confidence

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confidence was at once given to his declarations, the king advanced and gave him his hand. They abandoned the design of putting him and his companions to death, and from that moment treated him with the utmost friendship. Griffith and the Shawnees continued eight months in the nation; but were deterred from prosecuting their researches up the Missouri by the advice of the people of the country, who informed them, that they had gone a twelve month's journey up the river, but found it as large there as it was in their own country.

As to the history of this people, he could learn nothing satisfactory. The only account they could give was, that their forefathers had come up the river from a very distant country. They had no books, no records, no writings. They intermixed with no other people by marriage, there was not a dark-skinned man in the nation. Their numbers were very considerable. There was a continued range of settlements on the river, for fifty miles, and there were within this space three large water-courses which fell into the Missouri, on the banks of each of which they were likewise settled. He supposed that there must be fifty thousand men in the nation capable of bearing arms. Their cloathing was skins well dressed. Their houses were made of upright posts and the barks of trees. The only implement they had to cut them with, were stone tomahawks; they had no iron. Their arms were bows and arrows. They had some silver which had been hammered with stones into coarse ornaments, but it did not appear to be pure. They had neither horses, cattle, sheep, hogs, nor any domestic nor tame animals. They lived by hunting. He said nothing about their religion.

Griffith and his companions had some large iron tomahawks with them. With these they cut down a tree and prepared a canoe to return home in: But their tomahawks were so great a curiosity, and the people of the country were so eager to handle them, that their canoe was completed with very little labour. When this work was accomplished, they proposed to leave their new friends: Griffith, however, having promised to visit them again.

They descended the river with considerable speed, but amidst frequent dangers, from the rapidity of the current particularly when passing through the white mountains. When they reached

the Shawnees nation, they had been absent about two years and a half. Griffith supposed that when they travelled they went at the rate of about fifteen miles per day.

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He staid but a few months with the Indians after his return, as a favourable opportunity offered itself to him to reach his friends in Virginia. He came with a hunting party of Indians to the head-waters of Coal-river, which runs into New-river not far above the falls. There he left the Shawnees and easily reached the settlements on Roanoke.

Mr. Childs knew him before he was taken prisoner, and saw him a few days after his return, when he narrated to him the preceding circumstances. Griffith was universally regarded as a steady honest man, and a man of strict veracity. Mr. Childs has always placed the utmost confidence in his account of himself and his travels, and has no more doubt of the truth of his relation, than if he had seen the whole himself. Whether Griffith be still alive or not he does not know.

Whether his ideas be correct or not, we shall probably have a better opportunity of judging on the return of Captains Lewis and Clark; who, though they may not penetrate as far as Griffith alledged that he had done, will probably learn enough of the country to enable us to determine whether the account given by Griffith be fiction or truth.

I am, Sir,

Your humble servant,

HARRY TOULMIN.

Frankford, Dec. 12, 1804.

Additional Observations and Conjectures by the Editor.

THE story of a Welch colonization of America has excited much curiosity, both in Europe and the United States: By many it is believed, while by others it is thought unworthy of any attention. By reason of the present rapid progress of settlement in America, the time cannot be remote when the truth or falsity of this story will be completely established. In the meanwhile I do not hesitate to conjecture, *that no traces of the descendants of the Welch prince will ever be discovered in the western parts of North America.*

It

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It may not be improper to notice the tale upon which so many persons in Europe at least rest their hopes of proving, in the most satisfactory manner, that the Welch have contributed to the peopling of America.

David Powel, a Welch historian, informs us, that on the decease of Owen Guyneth, king of North Wales, a dispute arose among his sons concerning the succession to the crown; and that Madoc or Madog, one of the sons, "weary of this contention, betook himself to sea, in quest of a more quiet settlement *." We are informed, that "he steered due west, leaving Ireland to the north, and arrived in an unknown country, which appeared to him so desirable, that he returned to Wales, and carried hither several of his adherents and companions. After this neither Madog nor his companions were ever heard of more. The voyage of Madog is said to have been performed about the year 1170.

I have not seen Powel's work, but I learn that this historian, who lived in the reign of Queen Elizabeth, and consequently at a great distance of time from the event which he records, adduces no better authority in support of the voyage than a quotation from a Welch poet, "which proves no more than that he (Madog) had distinguished himself by sea and land †." Some few Welch words, such as *gwrando*, to hearken or listen, &c. are very feebly or unfortunately adduced by Powel, as circumstances favourable to the truth of the Welch emigration.

When we consider "that the Welch were never a naval people; that the age in which Madog lived was peculiarly ignorant in navigation;" that the compass was then unknown; the story of the voyages of the Welch prince must I think be considered as extremely improbable. I am of opinion with Mr. Pennant, that "the most which they could have attempted must have been a mere coasting voyage."

But it may be said, we must appeal to facts; and that independently of the verses of the Welch poet, and the arguments of the Welch historian, it seems highly probable that a colony of white people who speak the Welch language, does actually exist in the western parts of North America.

* Dr. Robertson.

† Pennant's Artic Zoology, Introduction, p. cclxiii. &c.

I cannot,

I cannot, I must confess, adopt this opinion. I readily allow, that the relations published by Mr. Toulmin and many other persons, both in Europe and in America, are extremely curious. But these relations are very inconsistent with one another, particularly in what relates to the actual state of improvement of the supposed Welshmen. By some we are told they are very far advanced in improvement; by others that their improvement is not at all greater than that of the Red-men or Indians of America. At one time, they are said to be in possession of manuscripts (and even printed books) at another time nothing of this kind is found among them. It must be confessed that Maurice Griffith's relation is, in several respects, more plausible than that of any preceding traveller; but it is not unincumbered with inconsistencies, which I do not deem it necessary to notice in this place. His assertion "that the white men of the Missouri speak pure Welch," even though this assertion be qualified by the observation that "they occasionally make use of a few words with which he was not acquainted," is to me one of the most improbable things that have ever been related of these people. His silence about their religion is altogether inexcusable. One would suppose that a person of Griffith's inquisitive turn of mind, would hardly have omitted to make some inquiries respecting the religious institutions of a people, whom he considered as his countrymen. If these people be the descendants of Madog, *some* traces of the Christian religion may be expected to be discerned among them; for I think it requires many centuries to entirely efface from the memory of a people all vestiges of their religion, especially from a people so tenacious of their language, and so little disposed to intermix with their neighbours, as the Welch Indians are represented to be.

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But Griffith's relation is, I think, worthy of some attention. I even think it not altogether improbable that future researches will establish the fact, that there does exist in the western parts of North Americ a race or nation of men, whose complexion is much fairer than that of the surrounding tribes of Indians, and who speak a language abounding in Welch or Celtic words. But the *complete* establishment of these two points would not prove the establishment of the truth of the assertion, that Prince Madog had ever made a voyage to America, or that

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that a colony of Celts had at any period prior to the *discovery of America by Columbus*, passed into this hemisphere *from Britain*.

It may be thought, from the statement published by Dr. Williams and some other writers on the subject, that the belief of the existence of a race of Welch Indians in America is generally admitted by the Welch Indians and others. But this is far from being the case. The late Mr. M'Gilivray, a man of no inconsiderable powers of mind, and whose curiosity was by no means confined to his own relatives, the Muscohge, or Creek Indians, informed me, in the year 1790, that he knew nothing of the existence of any white people in the tract of country beyond the Mississippi.

The following is an extract of a letter (dated *Downing, June* 14, 1792) from my learned and excellent friend the late Mr. Thomas Pennant of Wales.

"My countrymen are wild among the Padoucas, or Welch Indians, descendants of Madog, now seated about the upper parts of the Missouri. I am rather in disgrace, not having the warmest hopes of their discovery. Pray what is your opinion and that of your philosophers?"

In answer of the above I wrote a letter, of which the following is a part:

"I have heard a great deal about the Welch Indians. I very early imbibed your opinion, as delivered in your Arctic Zoology *, and mentioned you on the subject in a little work † which I published in England at the age of * * * *. I do not know whether you have seen that work. I do *not* mean to hint that it is worthy of your attention. I certainly think there is some foundation for the story; but I have no doubt but the whole affair will turn out very different from a discovery of Madog's descendants in America.

"I have said, that I think there is some ground for the story. I shall explain myself. You know that many of the first visitors of the new world were struck with the resemblance which

* See the introduction to the work, pages 263, 264.

† Observations on some parts of natural history; to which is prefixed an account of several remarkable vestiges, of an ancient date, which have been discovered in different parts of North America. Part I. London, 1797.

subsists

fubfifts between fome of the American nations and the Jews. Some Hebrew words were found in this continent, as they have been every where elfe. The Americans were now faid to be the defcendants of the Jews, and Adair laboured very hard to prove the matter in a ponderous quarto which few people read, becaufe it is big with fyftem and extravagance, though, indeed, it contains fome curious and accurate matter. In like manner, in the languages of fome of the American tribes there are found fome words which are a good deal analogous to words in the languages of the ancient Celts. Wafer, who was a very refpectable obferver, if we confider his occupation in life, mentions the coincidence he found between the language of the Indians of Darien and that of the Highland Scots; and I could produce inftances of their coincidence. Some Greek words are alfo found in certain of the American languages. I would not ftrain a point fo much as fome writers have, who mention the coincidence which fubfifts between the Greek *Theos* and the Mexican *Teotl.* The word *Potowmack*, which is the name of one of our great rivers, is a good deal like the Greek *Potomos* *. Thefe words (perhaps they are accidental refemblances) have given rife to fome of the numerous theories which we have had concerning the peopling of this great continent: and I doubt not that fome **** or perfon who underftood the Welch language, finding Celtic words (a language fpoken by the Welch) among the Americans, in the fulnefs of his zeal would bring his countrymen among the Padoucas, Apaches, &c.

Traditional account of inhabitants of America fuppofed to have originated from Wales.

" Such, I believe, has been the origin of this wonderful ftory. I prefume, that, were an ignorant Highlander to vifit the Darien Indians, or fome other American tribes, he would fancy himfelf among his countrymen, whom painting, expofure to the fun, &c. he might fuppofe had exalted or degraded to their prefent tinge. I lately converfed with an old Highlander, who faid, that the Indians fpeak the Highland language. Some Highland words were mentioned by him;

* The Abbé Molina (in his Compendio de la Hiftoria Civil del Reyno de Chile, &c. Parte Segunda, p. 334, 335.) has pointed out fome very ftriking inftances of refemblance between the Greek and Chilefe languages. He has alfo pointed out fome refemblance between the Latin and the Chilefe.—February 19, 1805.

**** one

**** one word **** I recollect, the word *teine*, which in the Highland language, he said, signifies fire: now our Delaware Indians call fire *teriday*; the resemblance in sound is certainly not small. The Celts have, undoubtedly, been very widely spread over the globe: I believe they existed in this country, and that their descendants are some of the present tribes*. That Celtic words should be found among the Americans, when Celtic words are to be found almost where else, is not I think to be wondered at."

IX.

Account of an improved Sheep-Fold, contrived and constructed by THOMAS PLOWMAN, *Esq. of Broom in Norfolk, and communicated by him to the Society for the Encouragement of Arts* †.

Advantages of the new sheep-fold.

THE model of Mr. Plowman's Sheepfold was forwarded to the Secretary of the Society of Arts last year with a letter describing its properties and construction. It is made on an improved and very simple principle, combining many advantages over the old and expensive method of folding by hurdles; and as the whole fold can be removed with ease at all times, it is found peculiarly useful in feeding off turnips on the land in frosty weather, when hurdles cannot be used; and, as the saving of labour in agriculture is a leading object, he has no doubt of seeing it, in a very few years, generally adopted.

Durability.

The expence, in the first instance, will exceed that of hurdles, for the same given quantity of sheep; but having had one in use nearly three years, he is satisfied the saving will be very considerable: for, before he adopted this method of folding, he lost from thirty to forty nights folding in the year, owing to the land being hard in dry seasons, such as the

* Very considerable fragments of the Celtic dialects are still preserved in America; particularly, if I do not mistake, among the Nanticokes and the Katalba or Katawbas. *February* 19, 1805.

† The Society awarded the gold medal for this useful improvement, and inserted his account.

two

two laſt; which renders folding almoſt impracticable, as they never can be ſet without great labour and deſtruction of hurdles. He is alſo clearly of opinion, that the ſtock of ſheep will be greatly increaſed when this method of folding becomes more known; and that it will enable many ſmall farmers to keep from 50 to 100 ſheep, who now are deterred from it, on account of the ſmall quantity of feed they have, not anſwering to keep a man for that purpoſe only; but by this plan, they may keep a boy at 3s. or 3s. 6d. per week, who can attend on 100 or 200 ſheep, and move the fold himſelf without any aſſiſtance. In heavy gales of wind it frequently happens that hurdles are blown down, and the ſheep, of courſe, being at liberty to range over the crops, do incalculable miſchief; which cannot happen with this fold.

Saving of hurdles; and greater profit in feeding and keeping ſheep.

It is eaſily moved, and not liable to be blown down;

In ſome counties in England, where hogs are folded, great difficulties are experienced for want of ſtowage, for them to feed off winter tares, &c. &c. as they root up every ſtake or hurdle; but from having tried the experiment, the inventor is certain his fold will keep them in, and defies their attempts to diſplace it.

reſiſts hogs.

From this drawing, which correſponds with the model, and from the deſcription, it is ſeen that an aſtoniſhing quantity of time is ſaved; for one man can remove a fold to contain 300 ſheep with eaſe in five minutes, which, by the old method, frequently takes ſome hours to accompliſh.

Certificates of gentlemen, who uſe theſe new folds, were ſent to the ſociety, among whom is that of his Grace the Duke of Bedford.

When the fold is wanted to be uſed on very hilly ground, it is beſt to begin at the top, and work it down to the bottom, for the eaſe of removing it, and then draw it up again with a horſe. This, however, the inventor has never had occaſion to do; for the land in his county is ploughed in a contrary direction, and the fold is worked in the ſame courſe as the ridges. By this mean, the inconvenience is avoided of croſſing the furrows, and they are alſo a guide to keep the fold in a ſtraight direction.

Method of placing it on hilly grounds.

With reſpect to the ſheep getting under, he does not recollect that circumſtance to have ever happened, nor does he conceive that any land, which is cultivated can be ſo uneven as to admit of it.

Description with reference to the drawing.

Description of the Sheepfold.

Plate XI. Fig. 1. Shows one division or part of this fence twenty-one feet long, and three feet eleven inches high, composed of the following parts:

A. A top rail three inches deep and two inches thick. B. The upper bar, three inches deep, and three-quarters inch thick. CC, The two lower bars, four inches by three-quarters of an inch, which, with the upper bar, are morticed through the uprights. DDDD, Which uprights are oak, three inches by two inches. E, The lower bar, three inches by three. F. An upright bar, with the horizontal bars halved into it. GG, Two oak uprights, three by two inches.

Fig. 2. Shows the oak uprights GG. H, The axletree, three inches by three, and three feet between the wheels. I, An oak knee, which connects the uprights GG with the axletree, by means of two screws and nuts.

Fig. 3. A plan, in which the axle H is shown with two arms KK at right angles to H, which are made to act as pivots to the wheels, when intended to be moved in a direction at right angles to the bars.

Fig. 4. Is a view of the same parts described in fig. 3. The wheels marked W, in all the figures, are of cast iron, and cost 3s. 6d. each.

X.

Anecdotes of an American Crow. By WILLIAM BARTRAM*.

Anecdotes of a crow.

IT is a difficult task to give a history of our crow. And I hesitate not to aver, that it would require the pen of a very able biographer to do justice to his talents.

Before I enter on this subject minutely, it may be necessary to remark, that we do not here speak of the crow collectively, as giving an account of the whole race, since I am convinced that these birds differ as widely as men do from each other in point of talents and acquirements, but of a particular kind of that species, which I reared from the nest.

* From the Philadelphia Medical Journal, Vol. I. part I.

He

He was, for a long time, comparatively, a helpless dependant creature, having a very small degree of activity or vivacity, every sense seeming to be asleep, or in embryo, until he had nearly attained his finished dimensions and figure, and the use of all his members. Then we were surprised and daily amused with the progressive developement of his senses, expanding and maturating as the wings of the youthful phalæna, when disengaged from its nympha shell. Anecdotes of a crow.

These senses however, seemed, as in man, to be only the organs or instruments of his intellectual powers, and of their effects, as directed towards the accomplishment of various designs and the gratification of the passions.

This was a bird of a happy temper and good disposition. He was tractable and benevolent, docile and humble, whilst his genius demonstrated extraordinary acuteness and lively sensations. All these good qualities were greatly in his favour, for they procured him friends and patrons, even among *men* whose society and regard contributed to illustrate the powers of his understanding. But what appeared most extraordinary, he seemed to have the wit to select and treasure up in his mind, and the sagacity to practice, that kind of knowledge which procured him the most advantage and profit.

He had great talents, and a strong propensity to imitation. When I was engaged in weeding in the garden, he would often fly to me, and after very attentively observing me in pulling up the small weeds and grass, he would fall to work, and with his strong beak pluck up the grass; and the more so, when I complimented him with encouraging expressions. He enjoyed great pleasure and amusement in seeing me write, and would attempt to take the pen out of my hand, and my spectacles from my nose. The latter article he was so pleased with, that I found it necessary to put them out of his reach when I had done using them. But one time, in particular, having left them a moment, the crow being then out of my sight, recollecting the bird's mischievous tricks, I returned quickly and found him upon the table, rifling my inkstand, books, and paper. When he saw me coming, he took up my spectacles and flew off with them. I found it vain to pretend to overtake him; but standing to observe his operations with my spectacles, I saw him settle down at the root of an apple-tree, where, after amusing himself for awhile, I observed

 that

Anecdotes of a crow.

that he was hiding them in the grass, and covering them with sticks and chips, often looking round about to see whether I was watching him. When he thought he had sufficiently secreted them, he turned about, advancing towards me at my call. When he had come near me, I ran towards the tree to regain my property. But he judging of my intentions by my actions, flew, and arriving there before me, picked them up again, and flew off with them into another apple tree. I now almost despaired of ever getting them again. However I returned back to a house a little distance off, and there secreting myself, I had a full view of him, and waited to see the event. After some time had elapsed, during which I heard a great noise and talk from him, of which I understood not a word; he left the tree with my spectacles dangling in his mouth, and alighted with them on the ground. After some time, and a great deal of caution and contrivance in choosing and rejecting different places, he hid them again, as he thought, very effectually in the grass, carrying and placing over them chips, dry leaves, &c. and often pushing them down with his bill. After he had finished this work, he flew up into a tree hard by, and there continued a long time talking to himself and making much noise; bragging, as I suppose, of his achievements. At last he returned to the house, where not finding me, he betook himself to other amusements. Having noted the place where he had hid my spectacles, I hastened thither, and after some time recovered them.

This bird had an excellent memory. He soon learned the name which we had given him, which was Tom; and would commonly come when he was called, unless engaged in some favourite amusement, or soon after correction; for when he had run to great lengths in mischief, I was under the necessity of whipping him, which I did with a little switch. He would in general bear correction with wonderful patience and humility, supplicating with piteous and penitent cries and actions. But sometimes when chastisement became intolerable, he would suddenly start off, and take refuge in the next tree. Here he would console himself with chattering and adjusting his feathers, if he was not lucky enough to carry off with him some of my property, such as a pen knife, or a piece of of paper; in this case he would boast and brag very loudly. At other times he would soon return, and with every token of penitence

penitence and fubmiffion approach me for forgivenefs and reconciliation. On thefe occafions he would fometimes return and fettle on the ground near my feet, and diffidently advance with foft foothing expreffions, and a fort of circumlocution, and fit filently by me for a confiderable time. At other times he would confidently come and fettle upon my fhoulder, and there folicit my favour and pardon with foothing expreffions and careffing gefticulations; not omitting to tickle me about the neck, ears, &c.

Anecdotes of a crow.

Tom appeared to be influenced by a lively fenfe of domination (an attribute prevalent in the animal creation) but neverthelefs his ambition, in this refpect, feemed to be moderated by a degree of reafon or reflection. He was certainly by no means tyrannical or cruel. It muft be confeffed, however, that he aimed to be mafter of every animal around him, in order to fecure his independence and his felf prefervation, and for the acquifition and defence of his natural rights. Yet in general he was peaceable and focial with all the animals about him.

He was the moft troublefome and teazing to a large dog whom he could never conquer. This old dog from natural fidelity and a particular attachment commonly lay down near me when I was at reft, reading or writing under the fhade of a pear-tree in the garden near the houfe. Tom (I believe from a paffion of jealoufy) would approach me with his ufual careffes and flattery, and after fecuring my notice and regard, he would addrefs the dog in fome degree of complaifance, and by words and actions; and if he could obtain accefs to him, would tickle him with his bill, jump upon him, and compofe himfelf for a little while. It was evident, however, that this feeming fociability was mere artifice to gain an opportunity to practice fome mifchievous trick, for no fooner did he perceive the old dog to be dozing, than he would be fure to pinch his lips, and pluck his beard. At length, however, thefe bold and hazardous achievments had nearly coft him his life, for one time the dog being highly provoked, he made fo fudden and fierce a fnap, that the crow narrowly efcaped with his head. After this Tom was wary, and ufed every caution and deliberation in his approaches, examining the dog's eyes and movements, to be fure that he was really afleep, and at laft would not venture nearer than his tail, and then by flow,

filent, and wary fteps, in a fideways or oblique manner, fpreading his legs and reaching forward. In this pofition he would pluck the long hairs of the dog's tail. But he would always take care to place his feet in fuch a manner to be ready to ftart off when the dog was roufed and fnapped at him.

It would be needlefs (obferves my ingenious friend in the conclufion of this entertaining account of the crow) to recount inftances of this bird's underftanding, cunning, and operations, which certainly exhibit inconteftible demonftrations of a regular combination of ideas, premeditation, reflection, and contrivance, which influenced his operations.

XI.

An Account of the Seiches of the Lake of Geneva. By M. Vaucher *.

Sudden and irregular rife and fall of the lake of Geneva called feiches.

THE inhabitants of the banks of the lake of Geneva, defignate by the name of feiches certain fudden and irregular changes which take place in the level of the waters of the lake, and have no relation with the regular and annual increafe produced by the melting of the fnows. This phenomenon was defcribed at the beginning of the laft century. Fatio de Duilers in the 2nd vol. of Spon's Hiftory of Geneva; and afterwards by Jalabert in the Academy of Sciences; Serre in the Journal de Savans, Bertrand, and by De Sauffure in the 1ft vol. of his Travels in the Alps. But though feveral of thefe philofophers have attempted to explain the fact, as we fhall hereafter remark, yet no one has confidered it with precifion, and as a general phenomenon. The editors of the Bulletin des Sciences, from whofe excellent fheet I take the prefent account, have followed Mr. Vaucher, and afterwards prefent the different explanations. The numerous obfervations of that philofopher have led him to the following general refults.

Particular detail of the facts; they are obferved in other lakes.

1. The feiches are not peculiar to the lake of Geneva, they are alfo obferved in the Lakes of Conftance, Zurich, Annecy, Neuf-chatel, and in the lake Major, and there are ftrong reafons to think that they exift in moft lakes, though they may not have been fufficiently obferved.

* From the Bulletin des Science, No. 96.

2. It

2. It appears, however, to be true, that the phenomenon is more remarkable in the lake of Geneva than any where else that it has been obſerved. In fact, the level of the waters of Leman lake have been ſeveral times obſerved to riſe at a given place in the courſe of 15 or 20 minutes, three, four, and even five feet, and to ſubſide ſome time afterwards, whereas the ſtrongeſt ſeiches obſerved in other lakes, have been four or five inches in the lake of Conſtance, eighteen lines in that of Zurich, four or five lines in that of Annecy, and only a few lines in the lake of Neuf-Chatel and lake Major.

But moſt ſtrikingly in the lake Leman.

3. In all theſe lakes, particularly in that of Geneva, the ſeiches are moſt ſenſible in that part of the lake which is neareſt the outlet of its waters. Accordingly they are no more than one or two inches, at the diſtance of two leagues from Geneva, and at the extremity near where the lake receives its waters the ſeiches of the lake of Geneva are not ſtronger than thoſe of the other lakes here mentioned.

More conſiderable near the place of efflux,

4. In theſe different lakes they are moſt ſenſible in places where the lake is remarkably narrow.

and where the ſhores are not far aſunder;

5. The ſeiches may take place indifferently at all ſeaſons of the year, and at any hour of the day; but in all the lakes it has been obſerved, that they are more frequent in the day than in the night, and in the ſpring and autumn, than in the winter or ſummer.

they happen at all times and ſeaſons;

6. It has been obſerved in particular in the neighbourhood of Geneva, that the ſtrongeſt ſeiches take place at the end of the ſummer, that is to ſay, at the time of the greateſt elevation of its waters.

but moſt ſtrikingly when the waters are higheſt;

7. The ſeiches are extremely frequent, but they are uſually a few lines, or at moſt only a few inches, in which caſes they cannot be perceived without exact apparatus to obſerve the level of the lake. It is from a want of this obſervation that they have been ſuppoſed to be very rare, as thoſe ſeiches only could be obſerved without apparatus which varied ſeveral feet.

8. The ſeiches take place without any agitation or motion of undulation or current in the ſurface of the fluid.

attended with no agitation,

9. Their duration is very variable, ſeldom exceeding twenty or twenty-five minutes, and often much leſs.

and do not laſt long;

10. This

they seem to be dependent on the weight of the atmosphere, and are thought to foretel rain.

10. This phenomenon takes place in all temperatures, but in general it results from very extensive tables, that the seiches are more frequent, and more extreme, the more variable the state of the atmosphere. Remarkable variations of the barometer have been observed to correspond with considerable seiches, and it is an opinion generally received among the fishermen, that the seiches are a sign of change of weather. In particular, they have been observed to be very strong when the sun comes to shine very strongly on a spot, a short time before obscured by a thick cloud.

Explanations by various authors.

After this exposition of the phenomenon, some notion may be formed respecting the value of the different explanations. M. Fatio attributes the seiches to violent gusts of wind which drive the waters into the narrowest part of the lake. Mr. Jalabert attributes them to some sudden encrease of the Arve, which falling into the Rhone at a short distance from the lake, and entering the river at a considerable angle, may in fact, sometimes stop its course for a short period, and in that manner raise the waters of the part of the lake nearest Geneva; lastly, Mr. Bertrand thinks this phenomenon to be occasioned by electrical clouds which attract the waters of the lake, and produce oscillations more sensible, the nearer its opposite banks may be to each other. Without dwelling on the insufficiency of these three hypothesis to account for all the different facts before mentioned; Mr. Vaucher observes, that the true explanation ought to be two-fold; namely, general in order to shew the cause of those less considerable seiches which are observed in all the lakes, and over the whole of their surface; the other must be local, and explain why this phenomenon is much more sensible at the western extremity of the lake of Geneva, than in any other known place.

Mr. Vaucher ascribes them to atmospheric pressure acting more strongly on one part of the lake than at the place of rise.

With respect to the first, Mr. Vaucher ascribes it to the frequent variations which are sensible in the weight of different columns of the atmosphere, and consequently in the pressure it exerts on different points of the surface of lakes *. We may easily conceive, that if the weight of the atmospheric column be speedily diminished in a given part of a lake, with-

* This cause was before indicated concisely by De Saussure, in his first vol. of Travels in the Alps.

out

out the ſame thing happening over the reſt of the ſurface; or ſtill more if the weight ſhould be augmented upon that remaining ſurface, the water will be forced to riſe in that laſt place, and will again deſcend when the atmoſphere ſhall have reſumed its equilibrium. It is known, in fact, that theſe variations of the barometer are ſo frequent, that it can never be ſaid to be exactly ſtationary: it is known, that they can be produced by changes of temperature, and De Sauſſure has calculated that a diminution of three degrees in the column of air will account for a variation of 0,85 of a line in the barometer. It is known, that theſe variations are moſt frequent in mountainous countries in autumn and in ſpring, and previous to ſtorms, circumſtances which coincide with the greater frequency of ſeiches at thoſe times. This general cauſe tends to explain the ſlight variations of level which are common to all the lakes; it is even of ſuch a nature as to be applicable to all extended ſurfaces, and it is therefore probable, that theſe variations of level likewiſe take place in the ſea, independant of the flux and reflux, which may have hitherto prevented their being obſerved. The variations in the weight of the atmoſphere may perhaps contribute to thoſe ſudden and local elevations of the waters of the ſea, which have all been indiſtinctly conſidered as of the nature of water-ſpouts. The ſame cauſe ought likewiſe to act on rivers, but inſtead of raiſing or diminiſhing their level, it ought, according to Mr. Vaucher, to produce a momentary acceleration or retardation of their courſe; an obſervation difficult to be made, and not hitherto attempted.

As to the ſecond part of the explanation, namely, that which ſhould account for the great intenſity of the phenomenon at the extremity of the Leman lake, near Geneva, Mr. Vaucher recurs to two circumſtances peculiar to that lake, and which are found in a leſs degree in thoſe of Zurich and Conſtance, where the ſeiches are moſt remarkable after thoſe of the lake of Geneva; namely, the contraction of a lake in a given place, and the deſcent of its waters towards the place of their diſcharge. With regard to the firſt of theſe circumſtances, it will be ſufficient, if attention be paid to a chart of the Leman lake, to ſhew that it is very remarkably contracted at its weſtern extremity, ſo that at half a league diſtance from Geneva, it has not one third of the breadth of that before Thonon. Now

—and he ſuppoſes the greater riſe in the lake of Geneva to be cauſed by its peculiar figure.

we

we may compare a lake of this form to a syphon full of water, of which the branches should very much differ in diameter; and it will be evident that if, for example, their inequality being as fourteen to one, the smallest branch should suddenly receive by the augmentation of the atmosphere a surcharge equal to that which depresses the barometer one line, it would fall 14 lines, and the water which would be driven into the great branch would raise its surface only one line; whereas, on the contrary, a surcharge which should depress the level of the great branch only one line, would raise it for a moment fourteen in the smaller. The effect would be double if at the same time the weight of the atmosphere should diminish on one of the branches, and encrease on one of the other. We may therefore admit that in lakes, the breadth of which is remarkably contracted in some part, the influence of the variations of the atmosphere to produce seiches will be greater in the narrow than in the wide part.

And also by circumstances attending the flowing off of the waters.

A like effect will take place according to Mr. Vaucher, by reason of the inclination observable in that part of the surface of the lake near the place where it discharges its water. He remarks that every particle of a liquid on a slope may be considered as solicited by two forces; one which tends to raise it to the level of the superior part of the slope or the reservoir, and the other which urges it in the direction of the current. If by the sudden depression of the superior fluid the current be for a moment suppressed, the particle will no longer find itself urged but by the first of these forces, and will rise towards its ancient level, and soon afterwards descend. Now, as we have before seen, all the parts of lakes which have very perceptible seiches have a remarkable slope; this slope is naturally more considerable at those times of the year when the waters are highest, and these are the periods when the seiches are most striking in the neighbourhood of Geneva.

Singular appearance which sometimes occurs that the surface of the lake is partly smooth and partly agitated.

Independent of the phenomenon of the seiches, the lake of Geneva and most other lakes afford two other singular phenomena; the one is known by the fishermen of the Leman lake by the name of fontaines. This takes place when the surface of the lake, instead of being uniformly calm or uniformly agitated, is seen to have certain parts calm and certain parts agitated, which are often mixed among each other in a thousand manners, and always very distinct. This fact seems to indicate

date the different atmofpheric columns, though very near each other, may fome of them be agitated and others calm. This appearance of the furface of the lake is confidered by the fishermen as a fign of rain.

The fecond phenomenon of which Mr. Vaucher fpeaks, confifts in certain fonorous diftant explofions or noifes which refemble thofe of the difcharge of artillery, and are fometimes heard in the fine fummer evenings. This phenomenon is rare, but is neverthelefs affirmed by feveral inhabitants near the lake of Geneva. It alfo takes place in the lake of Zurich according to Mr. Efcher, and in that of Baikal according to that of Mr. Patrin. Mr. Efcher afferts that half or three quarters of a minute after having heard one of thefe noifes he faw a bubble of air about a foot in diameter rife out of the lake of Zurich.

Another phenomenon refembling the diftant noife of artillery.

Annotations.—W. N.

It does not feem to me that any of the caufes yet pointed out are fufficient to account for the effect of the feiches. Sudden or ftrong blafts of wind could fcarcely operate in this way fo partially as that the exiftence of fuch fqualls fhould not at the fame time have fixed the attention of the common people as well as of the more accurate obfervers who have noticed thefe changes. It is perhaps equally difficult to fuppofe fuch unheeded variations to take place in the Arve fufficient to account for thefe very remarkable changes in the lake. Mr. Bertrand's electrical hypothefis refers us to a clafs of appearances too little underftood to be admitted, otherwife than in the way of loofe conjecture; befides which, it muft be remarked that the agency of electrical clouds is much more generally directed to mountains than to the valleys in which lakes muft neceffarily have their fituation. Much ingenuity is laftly fhewn by Mr. Vaucher in his explanation, which neverthelefs requires us to admit of atmofpheric columns confiderably differing in weight and occupying very fmall extent of furface. If this be even admitted as poffible, yet ftrong doubts may furely be entertained as to its probability. It appears to me that the object in queftion admits of an eafy folution upon other principles, and alfo that his explanation is grounded on pofitions not confiftent with the known laws of ftatics.

Objections to the theories which have been offered refpecting the feiches,

—particularly that of Mr. Vaucher's of atmofpheric preffure.

This ingenious author affumes as the conditions of his general theory that the lake fhould confift of two portions of water,

Recapitulation of his facts and deductions.

water, one much more extenfive than the other, and connected by a narrower portion or gut. He then ftates that if the atmofpheric preffure be greater upon the larger furface than on the fmaller, the firft will be depreffed and the latter will rife, and that the difference of elevation in each furface occafioned by the paffing of any given quantity of water will be greater the fmaller the furface.

No atmofpheric change can make a greater alteration in the lake than the correfpondent rife and fall of a water barometer, which is much lefs than really takes place.

This is very true; but it can in no cafe happen, that the difference between the level of one water and the other can amount to a greater quantity than that of a water barometer, by a like change, namely, about fourteen lines for every line of variation in the common barometer. That is to fay, if the barometer were to rife and fall again through half an inch, in the fhort time of a feiche, which I believe fcarcely if ever happens, the feiche itfelf could not rife above feven inches. The whole range of variation in the barometer could only caufe a rife of three feet and a half inftead of five which fometimes happens.

Another theory offered; that the feiches depend altogether on the rapidity of fupply and facility of difcharge.

I would venture to conjecture that this phenomenon is one among the numerous ofcillatory proceffes which take place when two variable natural powers are oppofed to each other in the production or modification of any event. Moft fmall lakes are formed by the enlargement of a river, by which the lake is fupplied at one end and evacuated at the other. The quantity of water in the lake itfelf will, in thefe circumftances, be always more than would be fufficient to fill its capacity, taken from the level of the loweft point of difcharge. How much more it may be than this quantity will depend upon the ftreams which enter and pafs out. An increafe in the quantity of fupply will keep the level higher, and fo likewife will any increafe in the obftacles to its flowing off; and on the contrary; if the fupply be diminifhed, or if the facility of difemboging be encreafed, the level will be depreffed. Thefe effects will take place moft ftrikingly at firft at that end of the lake where the efficient caufe operates. When any change has once taken place, fuch as that of the depreffion, it will continue for a fhort time after the caufe has ceafed to act; fo that the depreffion would itfelf be followed by a rife, even if the circumftances which caufed it were not alfo fubject to a like variation.

This effect is feen in brooks and mill-ponds.

Changes of this kind, on a fmall fcale, are obfervable in mill-dams, and even in the fmooth places in brooks or rivulets, as may

may be obferved by taking notice of fome part of the bank where a gently rifing fand may render the changes of level more confpicuous. The variablenefs of the weather at the fpring and autumn, by occafioning more frequent changes in the quantities of water, and confequently in the ftate of the rivers above and below the lakes in queftion muft render the feiches more frequent and extreme at thefe times. They will alfo be moft evident at the ends of a long lake; and the other circumftances will be modified by events that for the moft part would require to be afcertained by obfervations of the local circumftances and events on the fpot.

It muft be more confiderable in fpring and autumn.

The diftinct portions of rough and fmooth furface called *fontaines*, which are obferved on the lakes, are very ftrikingly feen at fea whenever a breeze fprings up after a dead calm. This effect is very remarkable, and may perhaps be accounted for on the fuppofition that the incipient motions of the air may be attended with eddies that may act more ftrongly on one part of the furface than another. This however does not feem reconcileable with a certain fteadinefs of appearance with which the fmooth and rough furfaces continue diftinct from each other for certain lengths of time. I am not much fatisfied with the conjecture which offered itfelf to me, or which may have been mentioned by fome other perfon when I was at fea many years ago; but it at leaft deferves to be noticed here. It is well known that the wind fcarcely takes hold of water which is covered with any oily film, and from the experiments of Franklin and others we have learned that a fingle drop of oil will rapidly fpread over a large furface of water, and caufe all the fmall primary waves to fubfide, rendering the furface extremely fmooth. It feemed to me not unlikely that oily matter from animal remains might rife to the furface of the fea during a calm and fpread itfelf irregularly over certain parts, which would continue fmooth for a confiderable time after the light commencing breeze had ruffled the other parts. I think from recollection that this appearance could not have lafted more than a quarter of an hour; but it is very common, and I often faw it. May not a fimilar caufe produce the appearance in the lake of Geneva.

The diftinct patches of fmooth and rough furface on the lake are very ufual at fea after a calm.

Suppofed to arife from oily matter on the furface of the water,

—of the lake alfo.

The fonorous reports refembling difcharges of artillery feem very likely to arife from the extrication of gas at the bottom of the water, which rifes and breaks at the furface. I have no

The fonorous reports refembling artillery fuppofed to be made by gafes.

no remark to make on this subject, but advert to it principally with a view to mention an effect not generally known, but calculated to shew the agitation which a small quantity of ascending air can produce in water. If a swimmer fill his lungs with air by inhaling as much as possible, and then dive down or descend into the water to the depth of fourteen, twenty or more feet, and when at that depth slowly blow the air out of his mouth, he will himself hear a roaring noise, and the spectators will see with surprise the surface of the water raised into a round or conical mass about a yard in height, with the water flowing round on all sides over a surface of seven or eight square feet. I have little doubt but that the noise of this rising column of water with the breaking of the bubbles of air would be very remarkable in one of the still evenings or nights of summer, when the effect of noises is remarkably more impressive than when the louder sounds of the day render them less observable, and in many instances altogether inaudible.

Remarkable effect of agitation in water, produced by air blown slowly through the lungs at the depth of several feet beneath the surface.

XII.

Experiments to ascertain the best Colour for marking the Heads of Pieces of Cotton or Linen in the rough, which shall be capable of resisting the Operations of Bleaching, as well as the most complicated Processes of Calico Printing, without spreading beyond the Limits of the Impression. By Mr. HAUFFMANN.*

IN order that a colour may be proper to mark piece goods of every kind it is requisite that it should contain no substance or drug capable of solution in alkalies; it is equally necessary that its component parts should not become white by oxygenation, and that they should remain insoluble in acids sufficiently strong for the bleaching processes, as well as for the operations antecedent to the calico printing.

Properties required in a good marking colour or ink.

Colours composed of drying oil cannot therefore, as I have found, be useful in these kind of marks, because they are not only attacked by alkaline and soapy liquids, but likewise because they dry slowly, and by spreading beyond the limits of impression, very often occasion spots.

Oil colours are bad because they yield to alkalies, &c.

* Annales de Chimie, LIII. 208.

If

If the colours of spirituous varnishes were not subject to the inconvenience of too speedy evaporation and drying they would be inadmissible on another account, namely, that the turpentine and resins are easily converted into soap. Gum copal is equally unfit for marking colours, because it quits the piece by simple ebullition in water. But as the varnish which I have made defends vessels of copper or any other metal from the action of acids of a certain strength as well as from that of the atmosphere, I have thought it might not be unacceptable to describe its composition in this place. To obtain this varnish from copal as pale and as clear as water, this gum must be reduced to very fine powder and exposed with twelve parts of the finest oil of turpentine for several days, or until it shall be completely dissolved at a moderate heat on a sand bath in a capsule of brass, stone ware, or porcelain, taking care to stir it as often as possible with a rod of glass. At the moment when the consistence of syrup begins to take place, the entire solution of the copal is effected by agitation, particularly if a small quantity of oil of turpentine be added from time to time to supply the loss by evaporation. Three fourths of the oil of turpentine which is lost by evaporation when open vessels are used, may be saved by performing the process in a long necked matrass, which is to be exposed on a sand bath a sufficient time to complete the solution of the copal, and shaking it very often. The varnish obtained by either of these methods becomes yellowish if the heat be urged too strongly; and as by its too glutinous consistence it would be difficult in its application, it is convenient, instead of diluting it with oil of turpentine, to mix it with one fourth or one fifth part of its weight of alcohol, taking care not to use too much, because an excess would render it of a milky white by the precipitation of part of the copal, which cannot admit in its solution more than a certain quantity of alcohol without precipitating. Vessels of brass or of any other metal may receive one, two, or three coatings of this varnish, and must be each time well dried in the oven. After this treatment they may be washed with boiling water without injury, and may even be exposed to a still greater heat without the varnish coming off; but these vessels must not be rubbed with sand or other hard bodies.

Varnish colours are equally faulty in this respect.

Copal yields to boiling water.

Process for making a good varnish. Copal in powder is dissolved by heat and agitation in oil of turpentine.

The copal varnish is to be diluted with alcohol.

Metallic vessels having this varnish baked upon them may be exposed to boiling water without injury.

By means of oil of turpentine, which evaporates and dries less speedily than alcohol, I succeeded in making a black composition

An oily compound for marking linen and other goods.

position, which I expected might be used with advantage in marking piece goods. For this purpose nothing more is needful than to dissolve slowly on the sand bath, and with constant agitation. One fourth of its weight of asphaltum or bitumen, judaicum well pounded, and afterwards to mix as much lamp black, or any other dark coloured mineral in fine powder, such as black lead, galena, or the like. This colour may be had more or less thick, by due proportions of the oil of turpentine and bitumen; it prints very well without running, if the proper proportions be attended to, and a little oil of turpentine be added when it becomes too thick. This bituminous colour supports the action of alkalies and of oxigen very well, and resists all acids of moderate strength.

As I thought it unnecessary to continue my experiments on oil colours, I made my experiments on watery compounds in the following order.

Section I.

First marking process. An impression is made of a solution of sulphate of manganese thickened with gum, and covered with lamp black, the cloth being then dipped in alkali, the manganese precipitates in brown oxide which affords a mark not to be discharged by bleaching, or by the printing processes.

I dissolved in four ounces of water one ounce of the sulphate of Manganese without its water of crystallization; that is to say, it was in the state it possesses when oxigen gas is procured from the black oxide of manganese, by means of the sulphuric acid, and by raising the heat to ignition at the end of the process. This solution was thickened with one dram of fine gum adragan in powder, and coloured with lamp black, in order to distinguish exactly the impression which may be easily made with this black saline metallic mass, of which nevertheless, we cannot make effective use without plunging the end of the marked piece into an alkaline ley, taking care that it shall not first be wetted with water, which would carry off the saline matter. The ley may be made with potash or soda, in the proportion of one part alkali to nine or twelve parts water. It may be used in the state of carbonate, or rendered caustic with half a part of quick lime. The precipitation of the oxide of manganese from the marks by either of these alkaline solutions will take place (exclusive of the stain from the lamp black) of a yellowish white colour, which will become more and more brown by attracting the oxigen of the atmosphere. The change of these marks to the brown, and even to a deeper colour inclining to black, will

will take place very ſpeedily by bleaching with the oxigenated alkaline muriatic ley, the pieces of which the ends have been plunged for a few minutes in the alkaline as before preſcribed. Theſe marks of the brown oxide of manganeſe reſiſt not only all the bleaching materials, and all acids of a requiſite force, but likewiſe the moſt complicated proceſs of manufacture of calico printing.

Section II.

The acetate of manganeſe cannot be uſed without the ſame manipulation, and as it is more coſtly it muſt be rejected.

If the acetic acid had not a much ſtronger affinity with manganeſe than it has with iron, and if it diſengaged itſelf as readily from the acetate of manganeſe as it does from the acetic ſolution of iron by evaporation and drying, we ſhould be able to procure indeliable marks in the moſt ſimple manner, by depoſiting the oxide of manganeſe on piece goods by means of the acetic acid, and afterwards ſimply leaving the oxide to the attraction and ſaturation of oxigen from the air. The acetic ſolution of manganeſe is very readily obtained by mixing a proper quantity of acetate of lead in a ſolution of ſulphate of manganeſe. But as this acetic ſolution affords no advantage in marking piece goods beyond thoſe of the ſulphate of manganeſe, and as it requires preciſely the ſame management as that deſcribed in the laſt ſection, and it is likewiſe more expenſive, it deſerves to be rejected.

Section III.

Sulphate of manganeſe with acetate of iron treated as before. It dries more quickly.

Two ounces of ſulphate of magneſia diſſolved in eight ounces of the acetic ſolution of iron, concentrated to the point indicated by twenty degrees, afford when thickened with one fortieth part of gum adraquack, a deep yellow liquor which becomes more and more brown, when treated abſolutely in the ſame manner as deſcribed in the firſt paragraph. The acetic ſolution does not, however, afford any other advantage but that of cauſing the marks to dry a little more ſpeedily; for the oxide of iron diſſolves in acids accordingly, as it is oxigenated. I give the preference to gum-adragant for thickening colours, to other gums and to ſtarch, becauſe theſe ſubſtances weaken the colours too much, if however, there ſhould be any objection to gum-adragan in coarſe goods, ſtarch may be then uſed.

 Section IV.

Section IV.

Marks printed with the grey oxide of manganese obtained in washing the residual sulphate, afford fixed marks by simple impression.

If care be taken in the process of disengaging oxigen gas from a mixture of the black oxide of manganese and sulphuric acid, not to carry the fire to ignition, the saline residue remains blackish, instead of becoming yellowish white by strong heat. When this residue is dissolved in water, it leaves behind it an oxide of a deep grey, which acquires a very pasty consistence on the filtre. This oxide mixed with a very little water thickened with gum adragant, may be used to print marks of a very deep grey, which dries speedily; and this colour does not wash out with water, even though the subsequent dipping in an alkali be omitted. It is so fixed that it not only supports the action of all acids of the manufacturing strengths, but likewise all the bleaching and printing processes without attracting the colouring matter of any dye whatever.

Section V.

Addition of the nitro muriate of tin to the marking oxide. It affords a dye.

If there were no reason to fear injuring in a slight degree the place where the mark is made, it would be advantageous to employ equal parts of the last described grey paste, and of a nitro muriatic solution of tin, containing one fourth part of the metal, and thickened with gum-adragath. This colour is as unalterable as that of the fourth section; and it has the additional advantage, that its oxide of tin being saturated with oxide of oxigen, attracts the colouring parts of any tincture, and acquires a puce colour by madder. I must observe on this occasion, that by the same madder dye, the colours of marks from the oxide of manganese saturated with oxigen, become of a deep puce colour, inclining to black, whereas in a less oxigenated state they acquire fainter shades. In all these circumstances however, it is requisite, that the quantity of metallic oxide should be as great as possible, otherwise the shades will be various, and less intense.

Section VI.

Experiments with the precipitate of manganese and solution of iron.

As many insoluble metallic oxides acquire the property of adhering to stuffs by means of acid, I did not fail to try whether the same would be the case with the precipitate of manganese saturated with oxigen. For this purpose I dissolved one part of

of ſulphate of manganeſe in fix parts of water, and precipitated the metal by adding to the point of ſaturation a cauſtic alkaline ley, made with half a part of quick lime, four parts of water, and one part of calcined potaſh of the ſhops. The precipitate was yellowiſh white. To the whole aqueous maſs I then added a ſufficient quantity of oxigenated muriatic alkaline ley, until the precipitate was completely ſaturated with oxigen, and its brown colour became no deeper. I afterwards collected on a filtre the precipitate or brown oxide of manganeſe, where, by the drainage of its water, it became paſty. This brown paſte, mixed with half its weight of the moſt concentrated acetic acid no longer afforded any but a weak browniſh ſhade; it was the ſame with a ſmall addition of one or the other of the three ancient mineral acids in a ſtate of ſolution. I did not ſucceed better by mixing one part of the ſame brown paſte with an equal quantity of the acetic ſolution of iron, marking 20° of the areometer of the ſaltpetre makers and thickened with gum adraganth. This acetic ſolution of iron containing only the quantity of oxigen neceſſary for the ſolution of the metal ceaſed by a ſtronger affinity, the exceſs of oxigen of the brown oxide of manganeſe, which in its turn became diſſolved, and the mixture of the two metallic ſolutions afforded a yellow reddiſh very deep and tranſparent liquid, which confirms the fact that a metal ſaturated with oxigen requires leſs acid for its ſolution than if it were in an oppoſite ſtate, and that being then furniſhed with an exceſs of acid, the ſolution ſaturated with oxigen can admit a portion of another metal without becoming turbid. This mixed ſolution of the two metals afforded me only a ruſty yellow which was diſcharged by weak ſulphuric acid completely, in ſomewhat leſs time than was required to take out a ruſt ſpot in a leſs oxigenated ſtate. In order to obtain from the mixture of theſe two metallic ſolutions an indelible marking colour, it is neceſſary that the marks ſhould be ſteeped for ſeveral minutes in an oxigenated muriatic alkaline ley, to precipitate and ſaturate the oxigen of the oxide of manganeſe. By mixing half a part of the brown paſte of manganeſe to two parts of the ſolution of the two metals the new portion remains untouched and renders the whole turbid. This turbid mixture left only a light browniſh mark on piece goods, which had remained long in the diluted ſulphuric acid.

Experiments with the precipitate of manganeſe and ſolution of iron.

The muriatic folution of tin takes up the oxides of iron and of manganefe.

By means of the muriatic folution of tin, which has the property of taking the oxigen from many vegetable, animal, and mineral fubftances, and which, on this account, is very ufeful in dying, as well as in calico-printing. We may difcolour and diffolve inftantly the deepeft oxide of manganefe and of iron, which proves the preponderating affinity of tin towards oxigen beyond that of manganefe or of iron.

N. B. There is no reafon to object to fteeping the marked goods in an alkaline ley; the operation is fpeedily made without fenfible lofs of potafh or of foda, if the operation of lixiviating be immediately proceeded upon, for which the remainder of the ley may be ufed. And generally, if the practice be ufed which has been adopted for a number of years, of rendering the alcalies cauftic with quick-lime, the faving will be confiderable and with better effect.

XIII.

Note on the Formation of Water by mere Compreffion; with Reflections on the Nature of the Electric Spark. By M. BIOT.*

That oxigen and hidrogen may unite by preffure.

IT was fome time ago that, in converfation with M. Berthollet on the nature and properties of heat, I communicated to him the perfuafion I had, that the combination of hidrogen and oxigen gafes might be determined without the aid of electricity, and merely by a very rapid compreffion. This refult appeared to me a confequence fo immediately following the obfervations already made on the heat difengaged from air by compreffion, that I thought it needlefs to afcertain it in any other manner. But having fince converfed with Mr. La Place, he appeared fo interefted as ftrongly to urge me to a verification. I therefore made the experiment, which completely fucceeded. It was made in the cabinet of the Polytechnic School. I am greatly indebted to M. Haffenfratz, profeffor of natural philofophy in that eftablifhment, for the

* Read to the National Inftitute of France, and inferted in the Annales de Chimie. LIII. 321.

great

great attention he paid in caufing the requifite preparations to be made, and for his perfonal affiftance in repeating it.

We took the fyringe of an air-gun, the bottom of which was clofed by a very thick glafs, in order that we might obferve the light difengaged as ufual by compreffion. This fyringe was of iron; it had a cock on one fide to introduce the gafes, and its lower extremity on the fide of the pifton was enveloped by a cylinder of lead, fufficiently weighty to accelerate the fall and render the compreffion more rapid. This apparatus was firft tried by introducing atmofpheric air; but though the experiment was made in the dark, no perceptible light was feen, probably becaufe the violent motion neceffary for the rapid compreffion, prevented the operator from looking fo directly through the glafs as to perceive the tranfient light which compreffion difengages, and which I myfelf had feveral times feen. Experiment in proof. The fyringe of an air-gun filled up fo as to receive the gafes and admit of infpection into its chamber, was firft tried with common air,

Immediately after this trial a mixture of hidrogen and oxigen gafes was introduced into the fyringe, and a ftroke was given. An extremely brilliant light appeared with a loud detonation: The glafs bottom was driven out: The copper fcrew which retained it in its place was broken; and the perfon who held the fyringe had his hand flightly burned and wounded by the force of the explofion. and afterwards with a mixture of oxigen and hidrogen. The glafs was broken by ftrong luminous explofion.

The experiment was repeated, by fubftituting a brafs bottom of one entire piece fcrewed on inftead of that of glafs, and a new mixture of the gafes was introduced. The firft ftroke of the pifton produced an explofion, which was heard like the loud crack of a whip; but a fecond ftroke with a new charge of the gafes, caufed a detonation which broke or rather tore the body of the fyringe with a violent explofion. Repetition with a metallic cap. The fyringe burft.

After thefe phenomena there can remain no doubt refpecting the combination of the two gafes; as it is known that this combination produces the detonation by the immenfe quantity of heat difengaged when they pafs to the liquid ftate; a heat which is fufficient to reduce them immediately into vapour, and give them an exceffive dilatation in that ftate. It was not therefore thought neceffary again to repeat this experiment, which is attended with fome danger.

The theory of thefe phenomena is extremely fimple. A rapid compreffion forces the gafes to abandon a very great Theory, the gafes give out heat.

quantity of heat, which not being capable of immediate diſſipation, raiſes their temperature in the inſtant ſufficiently to inflame them in this ſtate of compreſſion.

Thus it is that we find in the two gaſes all the elements neceſſary for their combination, independently of the electric ſpark or external heat. We might probably in the ſame manner, and without any foreign agent, produce all the gaſeous combinations which require an increaſe of temperature.

Deduction. The electric ſpark may conſiſt merely of light driven from compreſſed air.

This identity of reſults has led me to a notion which I ſubmit to the judgment of philoſophers. It is known, and M. Berthollet has ſhewn it in his Chemical Statics, that electricity in paſſing through bodies, produces a true compreſſion of their particles. This effect is produced with the moſt extreme velocity, as may be proved by an affinity of experiments. Now electricity poſſeſſing a velocity ſo great, it is impoſſible that it ſhould not diſengage light from the air, ſince we can diſengage it by a compreſſion ſo much leſs rapid. In this way it is that we are led to a concluſion, that this reſult of the electric ſpark is the purely mechanical effect of compreſſion.

More ample explanation. The extreme velocity of electric matter will ſtrongly compreſs free air,

If we now compare what paſſes in our condenſing pump and in the eudiometer of Volta, we ſhall find that the analogy is complete. Only that in the firſt caſe we are obliged to confine the air, becauſe the velocity we can give to the piſton is limited. Whereas in employing electricity, the particles are compreſſed by a velocity ſo great that they can never withdraw themſelves with ſufficient ſpeed from its effort. Therefore the compreſſion may be equally well made in the open air, together with the diſengagement of light or the ſpark, which is its conſequence. But this effect is local; and if the gaſes be not ſuſceptible of combining together, ſhould after each exploſion return to their primitive dimenſions, they muſt immediately reſume in this dilation all the heat they had before diſengaged, ſo that there cannot be effected any laſting change in their conſtitution. This explains why no alteration has ever been ſeen in very pure unmixed gaſes, when ſubjected to the action of the electric ſpark.

and alſo the rare fluids in our vacuum.

This light which electricity diſengages from the gaſes by compreſſion, it muſt alſo diſengage from the more rarified gaſes, and on account of its extreme velocity, it muſt diſengage it even from vapours, when experiments are made under the receiver

receiver of an air-pump or in the torricelian vacuum: For we can never form a perfect vacuum with our machines, and even in the tube of the barometer mercury always exists in the form of vapours. These vapours, though very rare, still contain a large quantity of caloric, which the electricity must disengage in its passage by compression; but the instantaneous augmentation of electricity which results, cannot become sensible on account of the little density of the medium; but this increase is perceivable in denser air, as we see in the instrument called Kinnersley's thermometer.

Conclusion.

The considerations which I have here made, appear to me to point out with some probability, that the phenomenon called the electric spark, is owing to the light disengaged from the air by compression during the passage of the electricity; so that this phenomenon is purely mechanical, and not at all electric in itself. This is the notion which I submit to the judgment of philosophers: if it be true, it must tend considerably to diminish the number of hypotheses which have already been made, or may be made on the nature of electricity. For this reason it is that I have offered it to their consideration, requesting that it may not be be thought that I consider it as of greater importance than their deliberate examination may bestow upon it.

XIV.

Account of Thermometers for registering the highest and lowest Temperatures in the Absence of the Observer. By F. A.

To Mr. NICHOLSON.

SIR,

Thermometers and barometers for registering the weather.

MANY contrivances have been proposed and adopted for registering all the stations of the thermometer and barometer, by means of a float or other equivalent instrument carrying a pencil, which marks its situation on a surface gradually moved along by means of a clock. These, of which meteorologists know the value, are nevertheless expensive, and require a degree of care and management sufficient to render simpler contrivances

Six's thermometer.

contrivances acceptable. Mr. James Six communicated, about 25 years ago, to the Royal Society a thermometer, in which two ſmall indicating pieces were driven by the fluid in the tubes to ſtations where they ſtuck, and remained after the change of temperature, and ſhewed the higheſt and loweſt degrees that had occurred ſince the laſt placing of them in contact with it. As this inſtrument is ſufficiently known, and I am now to advert to a ſimpler contrivance, I will diſmiſs that ſubject and advert to this laſt.

Objections to it.

In Mr. Six's complicated thermometer the tubes were vertical, and the indexes ſtuck in the glaſs by their ſpring; beſides which, a ſmall piece of ſteel wire being expoſed to alcohol, was at length oxided and ſet faſt.

Another thermometer, which ſhews the greateſt and leaſt temperatures.

The other contrivance now to be ſeen in all our London ſhops, and reſpecting which you will do an acceptable ſervice to your readers and the ſcientific world, by inſerting a ſketch in your Journal, conſiſts ſimply in two thermometers, one mercurial and the other of alcohol (*Fig.* 1, *Pl.* X.) having their ſtems horizontal; and the former has for its index a ſmall bit of magnetical ſteel wire, and the latter a minute thread of glaſs, having its two ends formed into ſmall knobs by fuſion in the flame of a candle.

Deſcription. It has a mercurial thermometer which ſhews the maximum; and a ſpirit thermometer for the minimum.

The magnetical bit of wire lies in the vacant ſpace of the mercurial thermometer, and is puſhed forward by the mercury whenever the temperature riſes and puſhes that fluid againſt it: but when the temperature falls and the fluid retires, this index is left behind, and conſequently ſhews the maximum. The other index, or bit of glaſs, lies in the tube of the ſpirit thermometer immerſed in the alcohol, and when the ſpirit retires by depreſſion of temperature, the index is carried along with it in apparent contact with its interior ſurface: but on increaſe of temperature the ſpirit goes forward and leaves the index, which therefore ſhews the minimum of temperature ſince it was ſet. As theſe indexes merely lie in the tubes, their reſiſtance to motion is altogether inconſiderable. The ſteel index is brought to the mercury by applying a magnet on the outſide of the tube, and the other is duly placed at the end of the column of alcohol by inclining the whole inſtrument.

Queſtion. Why the ſmall glaſs index always remains in the ſpirit.

I beg you will explain the motion of the glaſs index. I can eaſily underſtand from the general fact *that mercury repels ſteel*, that this fluid will drive the ſteel index before it; but I cannot make

make out to my ſatisfaction, how the ſpirit, by attracting glaſs, can prevent the other index from ever riſing out of its ſurface. Perhaps this thing may be already explained in elementary books; but whether it be or no, I am ſure that an account in your clear and popular way cannot be thought ſuperfluous.

I am reſpectfully,

Sir,

Your obliged

F. A.

REPLY.—W. N.

Explanation.

If the ſmall glaſs piece were protruded beyond the ſpirit, the fluid would hang to it and draw it back.

When the ſurface of the column of ſpirit is viewed by a magnifier, it is ſeen to have the form of a concave hemiſphere, which ſhews that the liquid is attracted by the glaſs. The glaſs in that place is conſequently attracted in the oppoſite direction by a force equal to that which is ſo employed in maintaining that concave figure; and if it were at liberty to move, it would be drawn back till the flat ſurface was reſtored. Let us ſuppoſe a ſmall ſtick or piece of glaſs to be looſe within the tube, and to protrude into the vacant ſpace beyond the ſurface of the alcohol. The fluid will be attracted alſo by this glaſs, and form a concave between its ſurface and that of the bore of the tube. But the ſmall interior piece being quite at liberty to move, will be drawn towards the ſpirit ſo long as the attractive force poſſeſſes any activity; that is, ſo long as any additional fluid hangs round the glaſs; or in other words, until the end of the ſtick of glaſs is even with the ſurface. Whence it is ſeen that the ſmall piece of glaſs will be reſiſted, in any action that may tend to protrude it beyond the ſurface of the fluid; and if this reſiſtance be greater than the force required to ſlide it along in the tube (as in fact it is), the piece muſt be ſlided along as the alcohol contracts; ſo as always to keep the piece within the fluid. And this fact is accordingly obſerved to take place.

Abſtract

XV.

Abstract of a Memoir on Milk. By M. THENARD.*

Component parts of milk.

IN a memoir which I read to the Philomatic Society in Praireal last, I shewed that milk always contains the free acetous acid in a greater or less quantity. At the same period Messrs. Fourcroy and Vauquelin found that it also contains phosphate of magnesia, and that the lactic acid of Scheele, or that which is obtained from serum of milk spontaneously coagulated, is merely the acid of vinegar combined with an animal matter. So that in the present state of our knowledge we must consider milk as composed of, 1. Water; 2 Acetous acid; 3. Caseous matter; 4. Butteraceous matter; 5. Sugar of Milk; 6. Extractive matter; 7. Muriate of soda and of potash; 8. Sulphate of potash; 9. Phosphate of lime; 10. Phosphate of magnesia.

Of these eleven substances there is one which I particularly examined some months ago, namely cream. I was desirous of ascertaining the circumstances which govern its separation, and particularly its transformation into butter.

The separation of cream and of butter does not require access of air.

I had before observed that milk coagulates as readily in closed as in open vessels; I know that no gas is disengaged in this decomposition, and that, in order to effect it with rapidity, it is needful only to raise the temperature to between 20° and 40° (Reaumur I suppose; and, if so, answering to 77° and 122° Fahrenheit). It was clear, therefore, that the air contributes neither to the formation nor the separation of cream, but that it exists ready formed in milk; but it remained to be shewn what are the principles which enter into its composition. Being persuaded, from various observations I had made, that it is only an intimate mixture of butter, cheese, and serum, I proceeded to ascertain this point by mixing a pint bottle (English quart) of recent cream nearly to its neck, from which I displaced the remaining air by carbonic acid. I then closed it well, and agitated it strongly in every direction for half an hour; at the end of which time the contents having

* Soc. Philomath, No. 96.

become

become very thick and adhering ſtrongly to the ſides of the bottle, gradually became detached, and ſoon afterwards were converted into a white liquid, in the midſt of which ſwam a yellow maſs of excellent butter. Hence it follows, that the butter exiſts in the milk, and is ſeparated when the milk, being deprived of the vital action, is left to itſelf. At this time, either by the formation of an acid ariſing doubtleſs from a decompoſition of the extractive matter, or perhaps from the leſs ſpecific gravity of the butter compared with that of the cheeſe; for the butter begins to ſeparate almoſt at the moment that milk is poured into a veſſel;—the milk is decompoſed, the cream riſes to the top, and from this laſt, by agitation, and more particularly by the aſſiſtance of a temperature between 15° and 20° (66° to 77° F.), butter is obtained together with butter-milk, which is a white very mild liquor, in which ſome butter and cheeſe are ſuſpended in a very divided ſtate. But the butter thus obtained is not pure: It ſtill contains a portion of cheeſe amounting ſometimes to the ſixth part of its weight; and this is the cauſe of its ſpeedily becoming rancid, particularly in ſummer. When the cheeſy matter is ſeparated by fuſion, the butter may be kept a long time. It is true indeed, that by this fuſion it acquires an acridneſs which greatly limits its uſes, and makes it unfit to be employed in frying; but this diſadvantage might be remedied by keeping the temperature much lower than is uſual. Clouet firſt made this obſervation; and hence the following proceſs may be adopted for purifying butter, or ſeparating the cheeſy matter without giving it a bad taſte.

Proceſs of butter making.

1. Let the butter be melted on the water bath, or at a degree of heat not exceeding the 60° of Reaumur. 2. Keep it melted till all the cheeſy matter is collected in white flakes at the bottom of the veſſel, and the melted butter is tranſparent. 3. At this period decant it, or paſs it through a cloth. 4. Let it be cooled in a mixture of equal parts of pounded ice and ſea-ſalt; or if ice cannot be procured, then in cold ſpring-water, making uſe of broad ſhallow veſſels. Without this precaution the butter would become lumpy by cryſtallizing, in which ſtate it could not be ſerved at table. Beſides which, the parts being condenſed by this ſudden cold, are found to reſiſt the action of the air more effectually. With this laſt intention it is alſo proper to

Purification of butter by fuſion, which ſeparates the cheeſy part.

to cover the pot in which the butter is kept very exactly, and to place it in a cold expofure, fuch as a cellar. By this treatment butter may be kept for fix months or more, and will be nearly as good as frefh butter, particularly after the top is taken off. It is even poffible to give this fufed butter to a certain point the appearance of frefh butter, by beating it with one fixth part of its weight of the cheefy matter; and fo likewife rancid butter may be confiderably amended by the procefs of fufion and cooling here prefcribed.

SCIENTIFIC NEWS.

Temperature of the Sea.

General facts refpecting the temperature of the fea.

MR. PIRON has lately communicated to the French National Inftitute a memoir on the temperature of the fea; an interefting fubject, capable of being applied to various ufeful purpofes, and which has accordingly engaged the attention of a confiderable number of philofophical obfervers. His general facts are, 1. The mean temperature of the fea at its furface is commonly more elevated than that of the air. 2. It is higher the nearer to the continents and large iflands. 3. At a diftance from the fhore in deep feas the water is colder below than at its furface; and the more the greater the depth. All the obfervations feem to fhew, that in the abyffes of the ocean, as well as on the fummits of mountains, even under the equator, eternal froft prevails. 4. A fimilar cold is obferved in extenfive lakes, and even within the earth at great depths, but it appears to be lefs fudden. 5. Thefe refults concur in proving, that the temperature within the earth is not every where the fame and equal to $9\frac{1}{4}°$, as has been long thought (about 50° Fahr. whether this be centigrade or Reaumur's fcale,)

At great depths it is eternally frozen.

Spent Oil of the Curriers.

Concerning the oil and compounds ufed in currying leather.

The procefs by which the curriers impregnate their fkins is is by fmearing the oil upon the wet fkin, into which it penetrates as the moifture evaporates. A pure oil could not perhaps be

be thus ſpread, and moſt probably would not enter the ſkin with the deſired effect, or render it as ſupple as that oil which from experience they are led to prefer.

The celebrated Seguin has directed his attention to this ingredient of ſuch extenſive manufactoring utility. He remarks, that this material (by the name of *Degras)* is of two kinds in France; *viz.* the common ſort and that of Niort. The firſt is the immediate product of the chamoying of ſkins, which are cleared of their ſurplus oil by ſolution of potaſh. It therefore contains not only ſoap, but likewiſe gelatine. It is evaporated to dryneſs and then ſold as *Degras.* At Niort it is decompoſed by ſulphuric acid, and the precipitate is called the *Degras* of that town.

Mr. Seguin finds by analyſis, that this laſt is oxigenated oil, whereas the other is a compound of ſoap and gelatine. He ſucceeded in giving to whale oil all the properties of the *Degras* of Niort, by boiling one pound for a few minutes with half an ounce of nitric acid at 25 degrees. He obſerved that no gas is diſengaged in this operation; but that water and nitrate of ammonia are formed; and he concludes that the oil was oxigenated, not by abſorbing the oxigen of the acid, but by yielding to it part of the hidrogen which was one of its own component parts. The reſult is the more intereſting, as the *Degras* of Niort being much more eſteemed than the common ſort, the curriers may hereafter, inſtead of paying a great price for it, make it in as large quantities as they pleaſe by following the proceſs here indicated.

Note reſpecting the Decompoſition of Sulphate of Lead by the Muriatic Acid. By M. DESCOTILS *.

If the ſulphate of lead be treated with muriatic acid rather concentrated, that metallic ſalt is totally diſſolved, provided the proportion of acid be rather in exceſs. This ſolution requires heat to effect it. Upon cooling, the muriate of lead cryſtallizes in great quantity; and it is much more ſpeedily obtained by the addition of a ſmall quantity of cold water. If the ſupernatant fluid be ſeparated from the cryſtallized ſalt, a pre-

Sulphate of lead is ſoluble by heat in muriatic acid; Muriate of lead ſeparates by cooling. This is ſoluble in water, and may be again decompoſed by ſulph. acid.

* Soc. Philom. No. 96.

a precipitate is obtained from the former by muriate of barytes. The muriate of lead is soluble in water, and may be almost entirely decomposed by sulphuric acid, which forms sulphate of lead.

Another instance in the analysis of antimonial galena.

This fact deserves to be carefully examined with relation to the play of affinities; and it may be of importance in the analysis of mineral and metallic substances. In fact, if an alloy contain a small quantity of lead, and it were necessary, in order to dissolve the alloy, to employ the nitro-muriatic acid, it would be very possible, and I have found it so, that sulphuric acid would not indicate the presence of lead. The following is another instance: If an antimonial galena be treated with nitric acid and sulphate of lead thus formed, this last would be decomposed by the muriatic oil (*qu.* acid?) which might be employed to take up the oxide of antimony, and the muriate of lead would remain dissolved after the addition of water. If care were not taken to examine the filtered liquor, a loss would be experienced which it would be difficult to account for.

Extract of a Letter from Naples, dated August 13.

Account of the late eruption of Mount Vesuvius.

"Yesterday at ten o'clock at night, the eruption of Vesuvius, of which the earthquake seemed to be the forerunner, took place. We were going to visit the crater when the cries of the people and a volume of flame informed us that the volcano had opened. The lava precipitated itself in three seconds from the last peak of the mountain and took a direction towards the valley, situated between Torre del Greco and Torre del l'Annunziata, two towns on the sea coast, beyond Portici, and seven or eight miles from Naples.

We set off immediately to see this wonderful and tremendous phenomenon nearer. From the place of our departure, we saw the whole course of the lava, which extended nearly two miles, from the crater to the houses that join the two towns. The sight was the most magnificently frightful that could be seen. I contemplated the cascades of flame pouring from the top of the mountain, and shuddered at seeing an immense torrent of fire ravage the finest fields, overthrow houses, and destroy in a few minutes the hopes and resources of a hundred families.

A line

Account of the late eruption of Mount Vesuvius.

A line of fire marked the profile of the mountain: a cloud of ſmoke, which ſeemed to ſend forth from time to time flaſhes of lightning, hung over the ſcene, and the moon appeared to be pale: Nothing can adequately deſcribe the grandeur of the ſcene, or give an accurate idea of the horror of it. As we approached the ſpot ravaged by this river of hell, ruined inhabitants having quitted their houſes—deſolated families trying to ſave their furniture and proviſions, laſt and feeble reſource—an immenſe croud of curious ſpectators, retreating ſtep by ſtep from the advancing lava, and teſtifying by extraordinary cries their wonder, fear, and pity—the frightful bellowing of the mountain, the frequent exploſions which burſt from the bottom of the torrent, the crackling of the trees devoured by the flames, the noiſe of the walls falling, and the lugubrious ſound of a bell, which the religious of the Camaldules, iſolated on a little hill and ſurrounded by two torrents of fire, rang in their diſtreſs. Such are the details of the frightful ſcene to which I was witneſs.

The moment we arrived the lava was croſſing the great road below Torre del Greco. To ſee it better we got into a beautiful houſe on the road ſide—from the terrace we ſaw the fire at no more than fifteen paces from us—in a minute we deſcended, and twenty minutes afterwards there remained of the houſe but three large walls. I approached as near as the heat and flow of the current would permit me, I attempted at different times to burn the end of my handkerchief in it—I could only do it by tying it to my cane. The lava does not run in liquid waves; it reſembles an immenſe quantity of coals on fire, which an invincible ſtrength had heaped up and puſhed on with violence. When it met with a wall, it collected to the height of ſeven or ten feet, burnt it, and overthrew it at once. I ſaw ſome walls get red hot, like iron, and melt, if I may uſe the expreſſion, into the lava. In its greateſt ſpeed and on an horizontal road, I reckoned that the torrent travelled at the rate of eighteen inches a minute. Its ſmell reſembled that of iron red hot.

Morning Chronicle.

Applicative

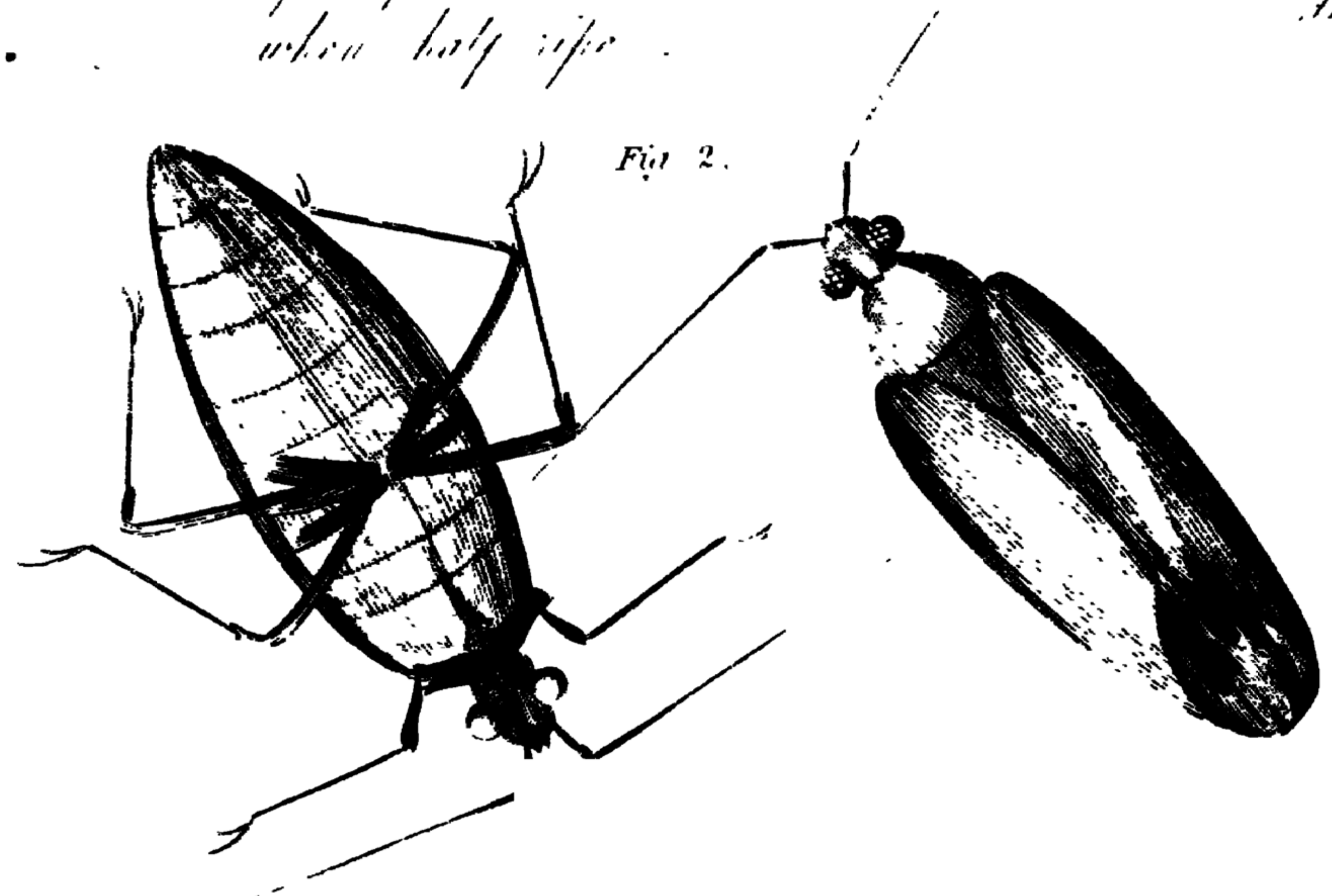
Applicative Compass for taking Bearings on a Chart, by N. D. STARCK, *Esq. of the Royal Navy.*

Compass for taking bearings, on a chart.

This instrument, seen in *Fig.* 6, *Plate* XI. consists of an inner and outer brass concentric circle; the latter of which, when in use, is to be applied to a chart, so that its cardinal points may agree with those of the draft, and its central (metallic) point be directly over the ship's place. The inner circle is to be set to the variation; and the thread from the center being laid, will shew either bearings by compass, or true bearings, according to the circle upon which they are read. It is obvious also, that the instrum nt may be used in delineatin , plotting, and for various other useful purposes.

Magnified representation of a green Insect often found in Corn Ears when half ripe.

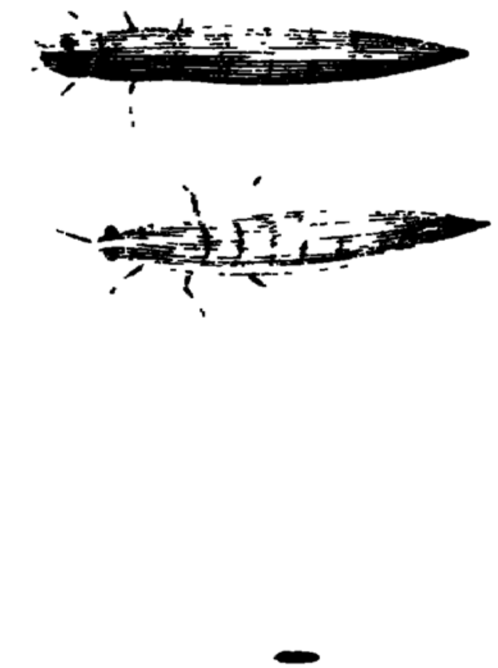
Fig. 2.



Natural Size

Pale yellow Insect found in diseased Grain magnified.

Fig. 1.



Natural Size.

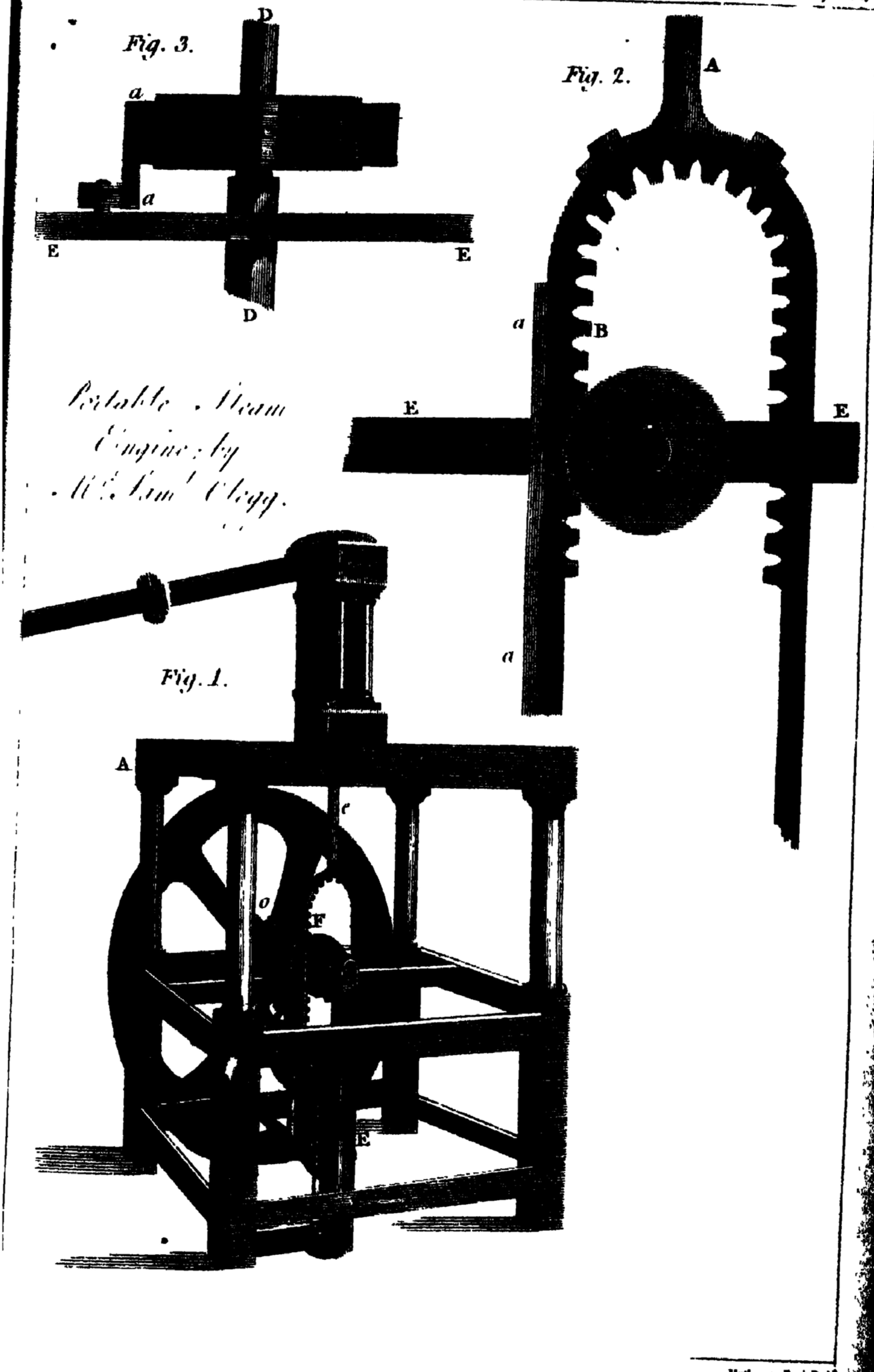


Fig. 3.
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Fig. 2.
A
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B
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E
E
Portable Steam Engine by Mr. Saml. Clegg.
Fig. 1.
A
e
o
F
E

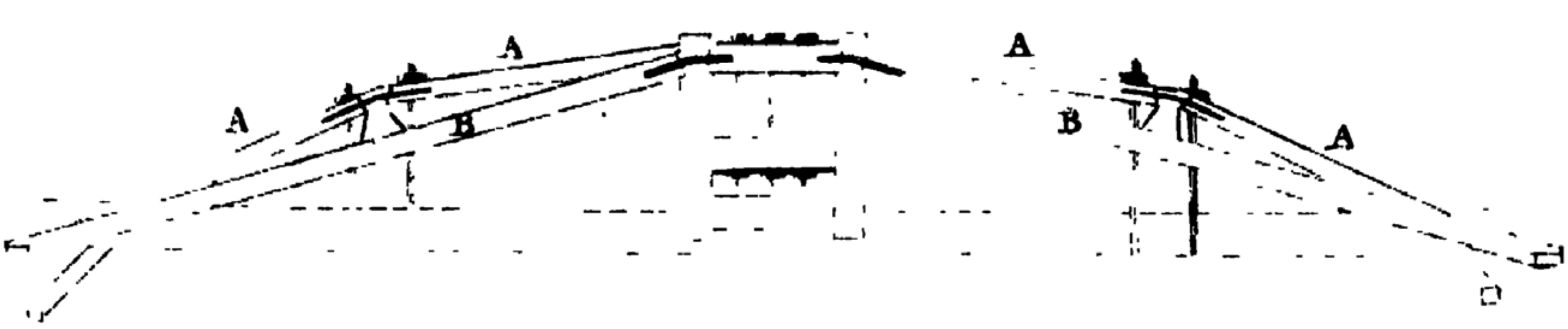
Self registering Thermometer.

Fig. 1.

Plan of Mr. Bleeces Truss by which the Roof of Clapham Church was raised.

Fig. 2.

Fig. 3



Mutlow sc.

Improved Sheep-fold
by T. Plowman Esq.r

Fig. 1.

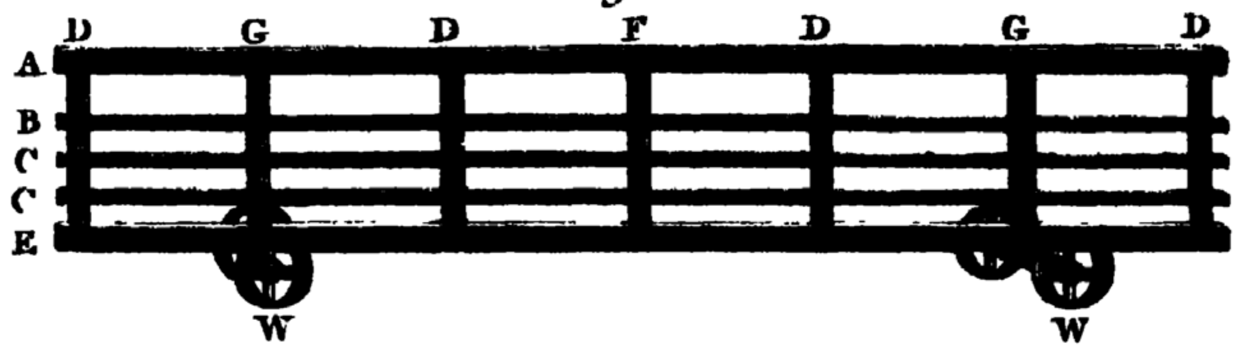


Fig. 3.

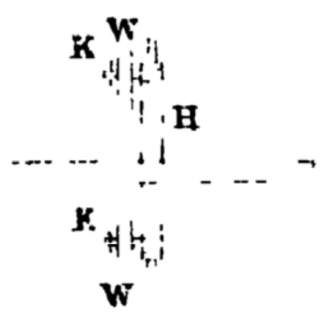


Fig. 2.

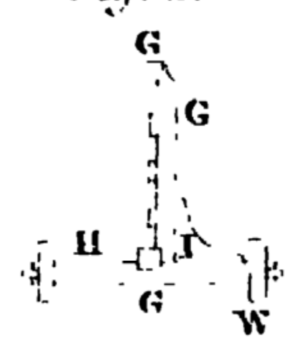
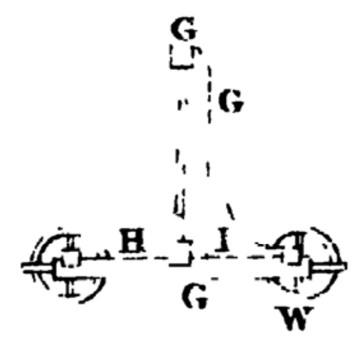
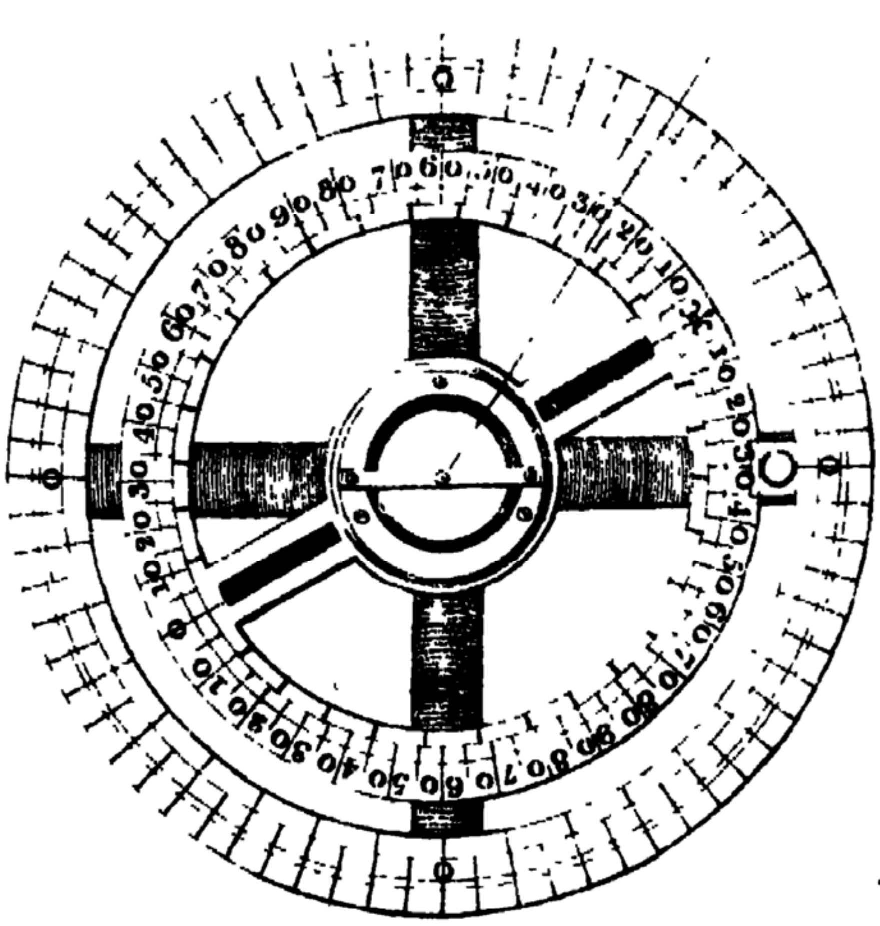


Fig. 4.



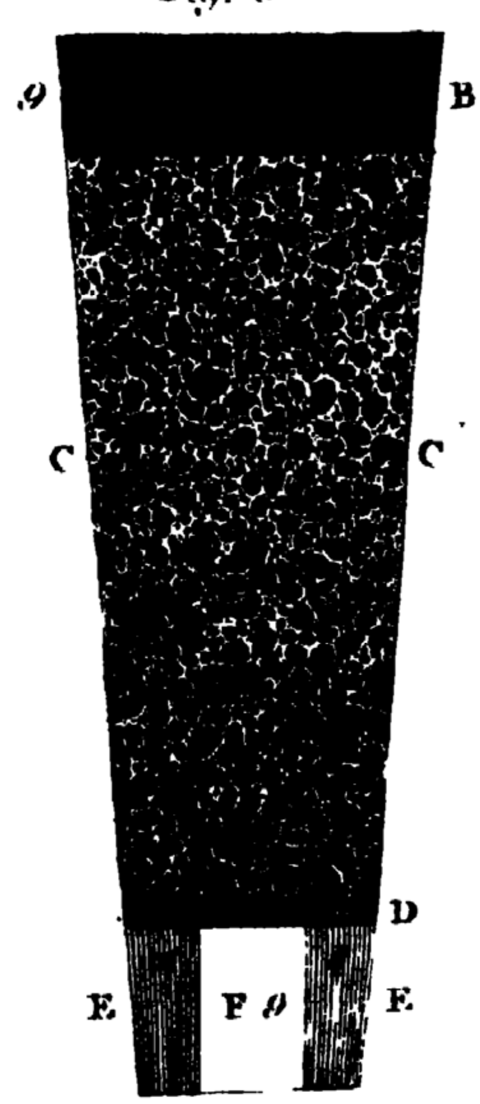
Applicative Compass
by N. D. Starck Esq. R.N.

Fig. 6



Drain, by
J. C. Curwen Esq.

Fig. 5



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1805.

ARTICLE I.

On the Division of an Arch of a Circle into two such Parts, that their Sines, or Cosines, or Versed-Sines, shall have a given Relation. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

BEING at present on a visit to my friend Michael Fryer, teacher of the mathematics at this place, I have availed myself of the opportunity to consult his very extensive mathematical library, with a view to discover how far the following theorems and problems are original; thinking it possible, at least, that similar propositions might be met with in the works of the early geometricians, particularly in the tracts on Angular Sections, by Vieta, Oughtred, Wallis, and others, which I had never before been able to meet with; but I have found only one of them to have been already treated, of which notice shall be taken in its proper place: nevertheless, it is not improbable but that similar theorems and problems are scattered up and down in the different works on geometry at present in existence: As this essay, however, may claim the merit of

Introductory letter.

exhibiting them in one view, and, which is equally desirable, of deriving them from a general principle, I have ventured to offer it for insertion in your Journal.

JOHN GOUGH.

Reeth, near Richmond, Yorkshire,
August 28.

Proposition I. Theorem.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Let A F be the arch of a circle, (*See Fig.* 1, *Pl.* XII.) A P a tangent at A; F P a perpendicular to A P, then A P is equal to the sine of A F; F P, the part of the perpendicular intercepted by the tangent and the point F in the arch, is equal to its versed sine; and the same line, P M, intercepted again by the circle in M, is equal to the versed sine of its supplement.

Demonstration.

Draw the diameter A K, and the sine F S perpendicular thereto; also from the center O, draw O L at right angles to P M; then, since P A touches the circle in A, P A K is a right angle, (Euc. 16. iij.) Also, the angles F P A, A S F, are right, by construction; therefore A S F P is a parallelogram, the opposite sides of which are equal, namely, A P = the sine S F, and P F = the versed sine A S, (Euc. 34. i.)

Again, since O L is perpendicular to P M, it is parallel to A P and S F, therefore P L = A O, or O K; and F L = S O, (Euc. 34. I.)—But F L = L M, (Euc. 3. III.) Consequently P M = S K, or the versed sine of the supplement A F. Q. E. D.

Proposition II. Theorem.

If A F B be an arch of a circle, (*See Fig.* 2.) and A P, B R, be tangents at A and B, from any point, F, in the circumference, draw F P, F R, perpendicular to the two tangents, and F Q also perpendicular to the chord A B, then will the rectangle $PF \times FR = \overline{FQ}|^2$; and the rectangles $AP \times BR$, and $AQ \times QB$, will also be equal.

Demonstration.

Join A F, F B, and the triangles P F A, Q F B, are equiangular, because they are right-angled at P and Q, by construction; and the angles P A F, Q B F, are equal, (Euc. 32. III.)

Therefore,

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Therefore, as A F : F B : : P F : F Q.

Also, the triangles Q F A, R F B, are equiangular, for the same reasons.

Therefore, as A F : F B : : F Q : F R.

Consequently, as P F : F Q : : F Q : F R, (Euc. 11. V.)

And $PF \times FR = \overline{FQ}|^2$, (Euc. 14. VI.)

Q. E. 1° D.

Again, by the same triangles, as F A : F B : : A P : B Q,

and as F A : F B : : A Q : B R;

hence, as A P : B Q : : A Q : B R,

Whence $AP \times BR = BQ \times AQ$:

Q. E. 2° D.

Corol. 1. Produce the perpendicular F Q till it meets the circumference again in G, and $PA \times RB = FQ \times QG$:

For $PA \times RB = AQ \times QB$ by the proposition; but $AQ \times QB = FQ \times QG$, (Euc. 35. III.)

Corol. 2. If the lines P F, R F, meet the circle again in M and N, then will $PM \times RN = \overline{QG}|^2$:

For $\overline{AP}|^2 = FP \times PM$, and $\overline{BR}|^2 = FR \times RN$, (by Euc. 36. III.)

Therefore, as $\overline{AP}|^2 : FP \times PM :: FR \times RN : \overline{BR}|^2$:

But $\overline{AP}|^2 : FQ \times QG :: FQ \times QG : \overline{BR}|^2$, by Corol. 1.

And P F : F Q : : F Q : F R, by the proposition.

Therefore,

$PF \times PM : FQ \times PM :: FQ \times RN : FR \times RN$.

Hence,

$FQ \times QG : FQ \times PM :: FQ \times RN : FQ \times QG$.

Consequently, $PM \times RN = \overline{QG}|^2$.

Corol. 3. Draw the diameters A K, B D, and make F S, F T perpendicular to A K, B D; then $AK \times BT$ (the rectangle of the versed sines) $= \overline{FQ}|^2$; $SF \times FT$ (the rectangle of the sines) $= AQ \times QB$; and $SK \times TD$ (the rectangle of the supplementary versed sines) $= \overline{QG}|^2$. These things follow from Props. I. and II.

Proposition III. Problem.

To divide a given arch of a circle (A B) into two parts (A F, F B), so that the rectangle of their versed sines (A S, B T) may be equal to a given magnitude, or square, $(m \times m)$.

Construction.

Division of an arch of a circle into two parts, having their fines, or cofines, or v. fines, in a given ratio.

From any point, Y, in the right line A B, draw Y W at right angles to the fame, making it equal to the given right line m; through W, parallel to Y B, draw W F, and let it cut the arch A B in F, then will A F, F B, be the required arches.

Demonftration.

Draw F Q perpendicular to Y B, then $FQ^2 = WY^2 = m \times m$; by Conft. and Euc. 34. I.; but the rectangle of the verfed fines of A F and F B $= \overline{FQ})^2$, (by Cor. 3. Prop. II.); therefore this rectangle is equal to $m \times m$, the given fquare. Q. E. D.

PROPOSITION IV. PROBLEM.

To divide A F B, a given arch of a circle, *(See Fig. 3.)* into two parts, A F, F B, fo that the rectangle of their fines may be equal to a given fquare, $(n \times n)$?

Conftruction.

To make the conftruction general, let A F B be greater than a femicircle, join A B, and in it take Q, making $AQ \times QB = n \times n$; alfo in A B produced take q, fo as to make $Aq \times qB = n \times n$; draw Q F, $q f g$, perpendicular to A B; then will A F, F B, or A g, g B, or A f, f B, be the required arches.

Demonftration.

This is evident from Cor. 3. Prop. II. and the conftruction.

To find *the limits*, bifect A B in Z, draw alfo the radius O N parallel to Z B, and make N E perpendicular to A B produced; then, if $n \times n$ be greater than $AZ \times ZB$, F is an imaginary point, becaufe $AQ \times QB$ cannot exceed $AZ \times ZB$, by Euc. 5. II. Again, if $n \times n$ be greater than $AE \times EB$, the points f, g, are imaginary, becaufe $Aq \times qB$ cannot exceed $AE \times EB$, feeing E N touches the circle in N, and is parallel to $q g$: Thefe things being premifed, it will be eafily perceived, that when A F B is lefs than a femicircle, it can only be divided in one point to anfwer the conditions of the queftion, becaufe the point N will be in the oppofite fegment; but when it exceeds a femicircle, it will admit of being divided into one, two, or three points, according to circumftances, or even the conftruction may prove impoffible. Q. E. D.

Scholium

Scholium.

This problem is conſtructed at page 342 of the Appendix to Simpſon's Algebra, 2d Edition; and at page 140 of his Select Exerciſes, 1ſt Edition; but the conſtructions given by that able geometrician do not ſhew the various limits of the queſtion with that degree of perſpicuity which appears in the preſent method.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines or v. ſines, in a given ratio.

Lemma.

Let A B C D be a ſquare, (*See Fig.* 4.) from any two adjacent ſides of which, C B, C D, take the ſegments T C, C S, then will the rectangle of the remaining ſegments $BT \times SD = \overline{BC}|^2 + TC \times CS - BC \times CT - BC \times CS$.

Demonſtration.

Draw S G, T H, parallel to B C, C D, and let them interſect in F;—

Then the rectangle $FTCS = TC \times CS$,

and the rectangle $FHAG = BT \times SD$,

But $FHAG + GBCS + HFSD =$ the ſquare A B C D; (Euc. 1. II.)

Add F T C S to both,

Then $FHAG + GBCS + TCDH = ABCD + FTCS$;

But C D is equal to B C,

Therefore $FHAG = ABCD + FTCS - BC \times CT - BC \times CS$;

That is, $BT \times SD = \overline{BC}|^2 + TC \times CS - BC \times CT - BC \times CS$. Q. E. D.

PROPOSITION V. PROBLEM.

To divide A F B, a given arch of a circle, (*See Fig.* 5.) into two parts, A F, and F B; ſo that the rectangle of their coſines may be equal to a given ſquare, $k \times k$?

Conſtruction.

Join A B, and from the center, O, draw O Z perpendicular to A B; in Z O take Z V equal to the given line, k, and join B V; draw the diameter, H h, parallel to A B, and divide it in I ſo as to make $HI \times Ih = \overline{BV}|^2$; from I draw I Q perpendicular to A B, and when produced let it meet the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Demonstration.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Let F I meet the circle again in G, draw the diameters A K, B D, and the sines F S, F T;

Then the cosine $OS = OA - AS$,

and the cosine $OT = OB - BT = OA - BT$;

Hence $SO \times OT = \overline{AO}|^2 + AS \times BT - AO \times AS - AO \times BT$;

But $AS \times BT = \overline{FQ}|^2$, by Prop. II.

Therefore,

$SO \times OT = \overline{AO}|^2 + \overline{FQ}|^2 - AO \times AS - AO \times BT$.

Again, $KS = 2AO - AS$,

and $DT = 2AO - BT$;

Hence, $KS \times DT = 4\overline{AO}|^2 + AS \times BT - 2AO \times AS - 2AO \times BT$;

But $KS \times DT = \overline{QG}|^2$, by Cor. 3. Prop. II.

Therefore,

$4\overline{AO}|^2 + \overline{FQ}|^2 - 2AO \times AS - 2AO \times BT = \overline{QG}|^2$;

Hence, $AO \times AS + AO \times BT = \overline{2AO}|^2 + \frac{\overline{FQ}|^2 - \overline{QG}|^2}{2}$:

But $SO \times OT = AO^2 + FQ^2 - AO \times AS - AO \times BT$

$= \frac{FQ^2}{2} + \frac{QG^2}{2} - \overline{AO}|^2$.

$FQ = FI - IQ = BP - OZ$,

and $GQ = GI + IQ = BV + OZ$;

Hence $\frac{FQ^2}{2} + \frac{GQ^2}{2} = \overline{BV}|^2 + \overline{OZ}|^2$;

Consequently, $SO \times OT = \overline{BV}|^2 + \overline{OZ}|^2 - \overline{AO}|^2$;

But $\overline{AO}|^2 - \overline{OZ}|^2 = \overline{BO}|^2 - \overline{OZ}|^2 = \overline{BZ}|^2$;

Therefore $SO \times OT = \overline{BV}|^2 - \overline{BZ}|^2 = \overline{VZ}|^2$,

(Euc. 47. I.) $= k \times k$, by construction. Q. E. D.

Limitation.—If Z V be greater than Z O, B V will be greater than B O; *i. e.* F G will be greater than H *h*, which is impossible, Euc. 15. III. therefore Z V, or *k*, cannot exceed Z O.

Proposition VI. Problem.

To divide A F B, a given arch of a circle, (*See Fig. 6.*) into two parts, so that the sum of their versed sines may be equal to a given right line, *u*?

Construction.

Construction.

Draw the radius A O, and the tangent B E; in A O take A I equal to the given line, u; and making I V perpendicular to A O, let it meet B E in V; draw F V to bisect the angle E V I, and let it cut the given arch in F; then will A F, F B, be the required arches.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Demonstration.

Draw the tangent A G, which is parallel to I V, also make F P, F R perpendicular to A G, B E, and let P F produced meet I V in H.

Then since A G, I V, are parallels, and the angle H P A is right, F H V is also a right angle, (by Euc. 29. I.) therefore it is equal to the angle F R V, (by construction.)

But the angles R V F, H V F, are also equal, (by construction); consequently the triangles R F V, H F V, are equiangular; and they have one side common, namely the side V F; therefore F R $=$ F H, (Euc. 4. VI.) and P F $+$ F R $=$ P H $=$ A I, (Euc. 34. I.) $= u$, (by construction.)

But the sum of the versed sines of A F, F B, is equal to P F $+$ F R, (by Prop. I.) therefore this sum is equal to the given line, u. Q. E. D.

Limitation.—If A I be greater than the versed sine of the whole arch A B, the point F will evidently fall in the opposite segment, and the construction will be impossible.

Again, since the angle I V E is equal to the angle A O B, draw the radius O C, to bisect the angle A O B, and it will evidently be perpendicular to V F; therefore L C, a tangent at C, will be parallel to V F; consequently if A I be so taken, that V may lie in B L produced, the construction will also be impossible; which will therefore happen when u is less than twice the versed sine of the arch A C, or B C.

Corol. Since the sum of the versed sines of two arches is the same with the difference of the diameter and the sum of the cosines, if the latter sum be given the problem may be constructed by the last proposition.

Proposition VII. Problem.

To divide A F B, a given arch of a circle, (*Fig.* 7.) into two parts, so that the sum of their sines may be equal to a given right line, w?

Construction.

Construction.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Draw the radii A O, O B, and the tangents A G, B E, in which take A S, B T, each equal to the half of w; draw S N, T N, parallel to A O, O B; and through their intersection, N, draw N F, parallel to S T, to meet the arch in F, then A F, F B, are the parts required.

Demonstration.

Draw F K, F M, parallel to N S, N T, and let them meet S T in K, M, and A G, B E, in P, R; then it is easily proved that the triangles K F M, S N T, are equal and similar, and that K M = S T; consequently S K = T M.

But the angles K P S, M R T, are right, being equal to the angles O A G, O B E, by construction; and the angles K S P, M T R, are equal; therefore the triangles P S K, R T M, are equiangular, they are therefore equal, (Euc. 4. VI.) because S K = T M; consequently S P = R T; therefore A P + B R = A S + B T = w.

But the sum of the sines of A F, F B = A P + B R; this sum is therefore equal to w.

Limitation.—Join A B, which will be parallel to S F, also let the radius O C bisect the angle A O B, when properly produced, or not, it will pass through the point N. Now if N be in O C produced, N F, being parallel to S T, or A B, will not meet the circle; on the other hand, if N lie between O and A B, F will be in the opposite segment of the circle, consequently the construction is impossible, unless N fall between C and the line A B, or in the versed sine of half the given arch: These things being premised, it will be easily perceived that the sine of the arch A F B is the less limit of the problem, and twice the sine of A C its greater limit.

Concerning

II.

Concerning the State in which the true Sap of Trees is deposited during Winter. By THOMAS ANDREW KNIGHT, *Esq**.

IT is well known that the fluid, generally called the sap in trees, ascends in the spring and summer from their roots, and that in the autumn and winter it is not, in any considerable quantity, found in them; and I have observed in a former paper, that this fluid rises wholly through the alburnum, or sap-wood. But Du Hamel and subsequent naturalists have proved, that trees contain another kind of sap, which they have called the true, or peculiar juice, or sap of the plant. Whence this fluid originates does not appear to have been agreed by naturalists; but I have offered some facts to prove that it is generated by the leaf †; and that it differs from the common aqueous sap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *suc propre*, and which I will call the true sap,) the whole substance, which is annually added to the tree, is derived. I shall endeavour in the present paper to prove that this fluid, in an inspissated state, or some concrete matter deposited by it, exists during the winter in the alburnum, and that from this fluid, or substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring, and thus furnishes those organs, which were not wanted during the winter, but which are essential to the further progress of vegetation.

The common sap rises in spring and summer, but not in winter.

True or peculiar sap

exists in the alburnum during winter: Its subsequent destination pointed out.

Few persons at all conversant with timber are ignorant, that the alburnum, or sap-wood of trees, which are felled in the autumn or winter, is much superior in quality to that of other trees of the same species, which are suffered to stand till the spring, or summer: it is at once more firm and tenacious in its texture, and more durable. This superiority in winter-felled wood has been generally attributed to the absence of the sap at that season; but the appearance and qualities of the wood

That winter or autumn felled wood has its alburnum more firm, &c.

commonly attributed to the absence of the sap:

* See Phil. Transf. of 1801, page 336.

† Philos. Trans. 1805, p. 88.

—but probably to its presence.

wood seem more justly to warrant the conclusion, that some substance has been added to, instead of taken from it, and many circumstances induced me to suspect that this substance is generated, and deposited within it, in the preceding summer and autumn.

Full grown leaves perspire most plentifully,

Du Hamel has remarked, and is evidently puzzled with the circumstance, that trees perspire more in the month of August, when the leaves are full grown, and when the annual shoots have ceased to elongate, than at any earlier period; and we cannot suppose the powers of vegetation to be thus actively employed, but in the execution of some very important operation. Bulbous and tuberous roots are almost wholly generated after the leaves and stems of the plants, to which they belong, have attained their full growth; and I have constantly found, in my practice as a farmer, that the produce of my meadows has been immensely increased when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the summer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the summer, in the preparation of matter calculated to afford food to the expanding buds and blossoms of the succeeding spring, and to enter into the composition of new organs of assimilation.

—and at this period the vegetative powers appear to be employed in increasing the growth of the vegetable.

If this be the case, it should be found that the aqueous sap must be altered in its ascent; and the winter felled wood will be denser.

If the preceding hypothesis be well founded, we may expect to find that some change will gradually take place in the qualities of the aqueous sap of trees during its ascent in the spring; and that any given portion of winter-felled wood will at the same time possess a greater degree of specific gravity, and yield a larger quantity of extractive matter, than the same quantity of wood which has been felled in the spring or in the early part of the summer. To ascertain these points I made the experiments, an account of which I have now the honour to lay before you.

Experiments. Birch and sycamore in spring gave sap most aqueous near the bottom; but denser and more saccharine the higher up.

As early in the last spring as the sap had risen in the sycamore and birch, I made incisions into the trunks of those trees, some close to the ground, and others at the elevation of seven feet, and I readily obtained from each incision as much sap as I wanted. Ascertaining the specific gravity of the sap of each tree, obtained at the different elevations, I found that of the sap of the sycamore with very little variation, in different

ferent trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

The sap first drawn was densest; which shews that its augmentation was had from matter in the alburnum.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

It is difficult to make experiments on the density or sp. gravity of the alburnum.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter

Method adopted. By felling poles in an oak coppice in winter and spring and comparing them.

lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age, and where many, of the same size and growing with equal vigour, spring from the same stool. One half of the poles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being left on all, and all being placed in the same situation to dry.

The winter felled wood was densest after seasoning,

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

—by more than ten per cent.

This difference of ten *per cent.* was considerably more than I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The difference was not quite so much in the newly formed layers of each.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.583, and that of the summer-felled to be 0.533; and trying the same experiment with similar pieces of wood, but taken from

poles

poles which had grown on a different ftool, the fpecific gravity of the winter-felled wood was 0.588, and that of the fummer-felled 0.534.

The winter felled wood gave out a larger portion of extract.

It is evident that the whole of the preceding difference in the fpecific gravity of the winter and fummer-felled wood might have arifen from a greater degree of contraction in the former kind, whilft drying; I therefore proceeded to afcertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when fteeped in water. Having therefore reduced to fmall fragments 1000 grains of each kind, I poured on each portion fix ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had funk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infufed, and had raifed its fpecific gravity to 1.002. The fpecific gravity of the water in which the fummer-felled wood had, in the fame manner, been infufed was 1.001. The wood in all the preceding cafes was taken from the upper parts of the poles, about eight feet from the ground.

Probability that this fap is exhaufted more or lefs by the leaves and fhoots.

Having obferved, in the preceding experiments, that the fap of the fycamore became fpecifically lighter when it had continued to flow during feveral days from the fame incifion, I concluded that the alburnum in the vicinity of fuch incifion had been deprived of a larger portion of its concrete or infpiffated fap than in other parts of the fame tree: and I therefore fufpected that I fhould find fimilar effects to have been produced by the young annual fhoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain lefs extractive matter than an equal portion taken from the lower parts of the fame pole, where no annual fhoots or leaves had been produced.

Experiment fhewed that they leave a certain portion of extract in the trunk.

No information could in this cafe be derived from the difference in the fpecific gravity of the wood; becaufe the fubftance of every tree is moft denfe and folid in the lower parts of its trunk; and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces feveral equal portions of wood taken from different parts of the fame poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I

fuffered

suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in the others that from the upper parts of the poles, had given to the water the deepest colour and greatest degree of specific gravity; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

Hence many trees have a succession of leaves and buds.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring: and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

The matter in the alburnum may remain inactive for several years.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases during several successive years: it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear tree, at a small distance from the trunk; and I pared off, at the same time, the whole of the lifeless external bark.

Instance: in an old pear tree.

The age of this tree, I have good reason to believe, somewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer; but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots, exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those which the tree had born in the three preceding seasons; and I

5 cannot

cannot believe that the matter which compoſed theſe buds and leaves could have been wholly prepared by the feeble vegetation and ſcanty foliage of the preceding year.

It is ſtrongly probable that this matter compoſes the leaf.

But whether the ſubſtance which is found in the alburnum of winter-felled trees, and which diſappears in part in the ſpring and early part of the ſummer, be generated in one or in ſeveral preceding years, there ſeem to be ſtrong grounds of probability, that this ſubſtance enters into the compoſition of the leaf: for we have abundant reaſon to believe that this organ is the principal agent of aſſimilation; and ſcarcely any thing can be more contrary to every concluſion we ſhould draw from analogical reaſoning and compariſon of the vegetable with the animal economy, or in itſelf more improbable, than that the leaf, or any other organ, ſhould ſingly prepare and aſſimilate immediately from the crude aqueous ſap, that matter which compoſes itſelf.

It is not likely that they are ſupported by the crude ſap.

It has been contended * that the buds themſelves contain the nutriment neceſſary for the minute unfolding leaves; but trees poſſeſs a power to reproduce their buds, and the matter neceſſary to form theſe buds muſt evidently be derived from ſome other ſource: nor does it appear probable that the young leaves very ſoon enter on this office: for the experiments of Ingenhouz prove that their action on the air which ſurrounds them is very eſſentially different from that of full grown leaves. It is true that buds in many inſtances will vegetate, and produce trees, when a very ſmall portion only of alburnum remains attached to them; but the firſt efforts of vegetation in ſuch buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this caſe, no grounds to ſuppoſe that the leaves derive their firſt nutriment from the crude ſap.

Seeds are thus nouriſhed not from the ſoil, but from matter depoſited in the cotyledons.

It is alſo generally admitted, from the experiments of Bonnet and Du Hamel, which I have repeated with the ſame reſult, that in the cotyledons of the ſeed is depoſited a quantity of nutriment for the bud, which every ſeed contains; and though no veſſels can be traced † which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous paſſage, which is perfectly ſimilar to that through which I conceive the ſap to be carried from the

* Thomſon's Chemiſtry. † Hedwig.

leaves

leaves to the buds, in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

(To be concluded in the Supplement.)

III.

On the Deliquescence and Efflorescence of Salts. By C. L. CADET.*

Deliquescence and efflorescence are occasioned by the relative attractions of the air and of each salt for water.

ALL chemists are of the same opinion relative to the cause of the deliquescence or the efflorescence of a salt. The attraction of the salt for the water contained in the atmosphere occasions the first phenomenon, the attraction of the atmospheric air for the water of crystallization of the salt causes the second.

The difference between one salt and another has been noticed, but not the variations from the state of the air, &c.

This attraction has been found to vary in the different salts, whether deliquescent or efflorescent, to be stronger in some kinds, and more speedy in others; but no one has yet observed whether it had any dependence on the constitution of the atmosphere, the electric state of the air, the quantity of caloric it contained, if it was always the same in any one salt, and if it regularly became weaker in proportion as saturation approached, neither have any tables been yet prepared, which might indicate the degree of deliquescence, or of efflorescence of the different salts.

Of the hypotheses which could be made on these phenomena, the following seemed most probable.

Hypotheses that deliquescent salts should attract water in proportion as the hygrometer indicated its presence.

The salts which deprived the air of its humidity ought to act in this respect in proportion to the quantity of water which the air held in solution or in suspension. The greater the humidity of the air, the more should the deliquescent salts augment in weight, so that the degree of their weight should be conformable to the progress of the hygrometer.

Barometrical changes and

On the other hand atmospheric pressure, which more or less opposes evaporation, ought to have an influence on the satura-

* Journal de Physique, LX.

tion

tion of the falts, fince it caufes the denfity of the air to vary; confequently there fhould be an agreement between the variations of the barometer and the deliquefcence of falts.

The variations of temperatures, by dilating, or by condenfing the mafs of the atmofphere, fhould alfo occafion changes in the proportion of water abforbed by the falts, on which account it would be ufeful to obferve the thermometer.

—thermometrical fhould alfo have their influence.

I thought moreover that one falt had not only more or lefs attraction for the water contained in the air than another, but that this attraction varied likewife in the fame falt in proportion as it had loft or abforbed water. I hoped by thus comparing the deliquefcence and efflorefcence of falts with the ftate of the different meteorological inftruments, to obtain refults fufficiently conftant to eftablifh a theory of deliquefcence or efflorefcence. I hoped alfo to be able to ufe the falts themfelves as inftruments of meteorological obfervation; but experience proved that reafoning apparently founded on the trueft theory frequently deceives expectation. It is neverthelefs neceffary to attend to negative facts, which fometimes are as ferviceable to fcience as thofe of a pofitive nature.

Deliquefcent falts fhould attract moft when leaft faturated.

Experience did not confirm thefe pofitions.

I did not find a fingle falt which feemed to have the leaft conformity with the ftate of the barometer, hygrometer, or thermometer. On the fame day many falts increafed confiderably in weight, while others indicated a flow progrefs. Some had but a fmall attraction, when the hygrometer fhewed a great degree of humidity, and were moft deliquefcent when the air feemed moft dry. Atmofpheric preffure never had the leaft agreement with the increafe of weight of a falt, and the thermometer having varied but half a degree during the courfe of the experiments, does not furnifh any obfervation on the influence of temperature. It is therefore impoffible to explain by the meteorological changes any of the variations which I obferved in the deliquefcence, or the efflorefcence of falts.

None of the falts appeared to gain or lofe weight in conformity to meteoric changes.

Efflorefcent Salts.

I weighed exactly 288 grains of fulphate of foda, of phofphate of foda, and of carbonate of foda, which three falts are confidered as the moft efflorefcent, and placed them in a dry and airy fituation, after having carefully dried the capfules which contained them. I put alfo in the fame place an hygrometer, a barometer, and a thermometer: the three falts fhewed the following refults.

Experiments. Efflorefcent falts, fulphate, phofphate and carbonate of foda expofed.

 Sulphate

Loſs of weight in each by effloreſcence.

	Left to effloreſce.	Loſt.
Sulphate of ſoda	61 days	203 grains of water.
Phoſphate of ſoda	39	91
Carbonate of ſoda	51	86

Conſiderations why the time ſeems to indicate no uſeful reſult in deliqueſcent ſalts expoſed.

It ſhould ſeem from this table that theſe three ſalts ought to be claſſed in the preceding order; but it muſt be obſerved that ſalts contain more or leſs water, in proportion as they cryſtalize ſlowly or rapidly. The number of days which were employed in the effloreſcence of theſe ſalts ſhould vary, both in proportion to the water they contained, and to the extent of ſurface which they expoſed to the action of the ſurrounding air; and therefore the time of their effloreſcence can give no appreciation of the force of their attraction for water. This reflection prevented my making experiments on any more effloreſcent ſalts.

Deliqueſcent Salts.

I took 288 grains of each of the ſalts in the following table, (which are very ſenſibly deliqueſcent, ſince they all abſorbed more than half their weight of water), and placed each of them in a dried capſule, along with the before-mentioned meteorological inſtruments, in a damp ſituation, and after 150 days of obſervations noted what is included in the table.

A Table of Deliqueſcent Salts, in the Order of their Attraction, eſtimated by the Quantity of Water abſorbed.

Table of the increaſe of weight in each of 19 different ſpecies, and the times reſpectively.

	Days employed in their ſaturation.	Water abſorbed.
Acetite of potaſh	146	700 grains.
Muriate of lime	124	684
Muriate of mangeneſe	105	629
Nitrate of manganeſe	89	527
Nitrate of zinc	124	495
Nitrate of lime	147	448
Muriate of magneſia	139	441
Nitrate of copper	128	397
Muriate of antimony	124	388
Muriate of alumine	149	342
Nitrate of alumine	147	300
Muriate of zinc	76	294
Nitrate of ſoda	137	257
Nitrate of magneſia	73	207

	Days employed in their saturation.	Water absorbed.
Acetite of alumine	104	202
Acid sulphate of alumine	121	202
Muriate of bismuth	114	174
Acid phosphate of lime	93	155
Muriate of copper	119	148

In examining this table it may be remarked that the duration of the absorption is not in any proportion to the quantity: The muriate of alumine, for example, took 149 days to absorb 342 grains of water, while the nitrate of manganese took but 89 days to absorb 527 grains. That the force of attraction may be estimated from the rapidity with which the bodies unite must not be concluded from this; for the same table shews that nitrate of magnesia saturated itself in 73 days, and only absorbed 207 grains of water, a much less quantity than that taken up by the nitrate of manganese. Although the greater or less facility with which deliquescent salts saturate themselves with water cannot be accounted for, (since a salt half saturated, or half deprived of water, is no longer the same body, and consequently exercises other attractions than what the same salt does in its ordinary state, or in a different state of saturation,) the rapidity of their saturation is not however an indifferent matter. In the experiments which have been made on producing artificial cold by muriate of lime, it has been remarked that the cold was greater in proportion as the ice was melted; but it is probable that the muriate, and above all the nitrate of manganese, which becomes liquid much quicker, would produce with ice a more intense cold, and that certain liquors which have hitherto resisted coagulation, would be solidified by these two salts, which experiment is highly deserving of a trial.

The times of absorption were not at all proportional to the quantities.

Though the rapidity of absorption does not indicate the proportional affinity yet the facts are useful.

For instance, the salts of manganese may produce intense refrigeration.

In order to examine whether deliquescence depends on the proportion of the base, or of the acid which constitutes the salts, I compared with each other the different analyses of salts published by Bergman, Klaproth, Fourcroy, and Vauquelin, and I saw that no induction could be from their composition; for there are some salts which have the base in a very considerable proportion, and which are less deliquescent than those whose base is less; and many others in which the acid is in a small proportion, are more deliquescent than those, in which this principle is predominant. The nature of the acids and of the

It does not appear that the deliquescence depends on the proportion of component parts,

—nor on the peculiar nature of the ingredients themselves.

the bases themselves do not throw more light on the phenomena of deliquescence than their proportions; for there are deliquescent salts, the component parts of which taken separately, have not any remarkable attraction for water, such is the nitrate of alumine; while on the other hand the sulphate of soda is efflorescent, although concentrated sulphuric acid, and caustic soda each separately attract humidity. Nothing better proves this axiom in chemistry,—*Compounds have properties peculiar to themselves, and differing from those of their component parts.*

Generally the deliquescence was most rapid when the saturation was least.

In general deliquescent salts encrease their weight in a diminishing proportion, according as they approach saturation; thus the acetate of potash, which in the first twenty days exhibited the following progression: 21. 34. 44. 54. 60. 70. 85. 100. 110. 120. 128. 138. 142. 148. 160. 169. 177. 186. 192. 198. did not shew on the last twenty days more than this, 647. 650. 655. 660. 663. 666. 669. 671. 676. 682. 684. 686. 688. 690. 692. 694. 696. 698. 699. 670. The salts which were but little deliquescent presented a singular phenomenon, which none, I believe, has observed before.

Remarkable facts; salts which lose part of the absorbed water and afterwards attract more and encrease till saturation.

The acid sulphate of alumine, and the acid phosphate of lime, increased and diminished successively in weight.

The muriate of copper diminished during 45 days before it began to encrease. These oscillations and retrograde movements take place but once, and when the salt has absorbed a certain quantity of water, there is a progressive increase, although slowly, until its perfect saturation, which may depend on the attraction of water for water, an attraction which is not perceptible but in certain proportions.

Expediency of further experiments.

These anomalies deserve to be observed again, and compared with experiments made on other salts which do not exhibit them. They tend to make us acquainted with all the causes that produce efflorescence and deliquescence, since they present each phenomenon successively. The salts which we submitted to their action, had certainly an attraction for water very little different from that of air in a medium state of heat and humidity. The point of equilibrium must be decided by the state of the atmosphere, or the salts would remain unaltered.

These will probably shew that meteoric variations in the air

I still however think that a relation exists between the meteorological variations and the alterations of the salts; and if I was

was not able to discover it, without doubt this was caused by the small portions of salts which I exposed to the action of the atmosphere. Some chemist more fortunate will determine it, by operating on large masses, comparing experiments made in many different seasons, and keeping a register of the electrical state of the atmosphere, of the water of crystallization which the salts contain, of their division, and of the surface which they present to the air. do influence th changes in salts

In a labour which would require more than 3000 experiments, the new facts which I have observed are too few, and perhaps too little important to engage any one to undertake such prolonged and minute experiments; but I have given a table of deliquescent salts arranged according to their attraction for water, and I dare hope, that the results of it will not be altogether useless. Extensive research.

IV.

Account of the simple and easy Means by which the Harbour of Rye was restored, and made navigable for Ships of considerable Burthen. By the Rev. DANIEL PAPE *.

Memorial of Rye Harbour.

RYE Harbour, once so very safe and convenient for passing vessels up or down the channel, to run to in distress or in precarious weather, had been for many years, and from various causes, in a gradual state of decay, insomuch that in the years (I believe) 1795 and 1796, it was thought necessary to send Captain ———, from the Trinity-House, to make a survey, and report to the Board its then state, and the probability of its improvement or redemption. The survey was made, I believe, with considerable care and attention; and the result was, that the harbour was pronounced lost, or in such an irreparable decayed state, that it was an useless expense to the ships passing, which paid tonnage to it; and therefore this tonnage was taken from Rye, and given to Ramsgate Harbour, leaving however a reserve in the hands of the commissioners of 6000*l*. Decayed state of Rye Harbour, in 1796. Survey and report, that the Harbour was irreparable.

* From his communication to the Society of Arts, who voted him the gold medal. See their Transactions, Vol. XXII.

The

Advertisement for plans of improvement.

The consequence of this was an advertisement, inviting any gentleman to come forward with plans for the improvement of the harbour, and the draining of the upper levels. On the day appointed for the presentation of such plans, a very sensible letter was laid before the Commissioners by the Rev. Mr. Jackson, of Rye, though impracticable on many accounts,—and also a plan by myself, proposing to make the present cut, and to form a dam of straw or hay and faggots, as represented on the chart, for the small sum of 500*l.* On reverting to the *enormous sums* that had been already, from time to time, expended by able engineers to no purpose, it was judged at the moment an impossible attempt; and, after politely voting me their thanks, the Commissioners seemed to decline carrying their plan into execution.—This, however, did not satisfy me; and therefore, confident of success, I undertook to perform what I had proposed, or lose the money, without stipulating for any fee or reward should I succeed. On entering upon this agreement, I set to work, and choosing a Mr. Southerden, an active and persevering man, as foreman, to assist me, I completed the work in three months, in the very depth of winter, at the expense of only 480*l.* though the works were twice filled up with sea-beach by the tides. But, though this was done to the astonishment and admiration of many, yet there were evidently an envious few mortified and disappointed. The cut and dam being thus finished, it was then thought necessary, on my recommendation, to secure the cut from reverting to its late reduced state, by a pier-head on the east, and jutties on the west side of it; the execution of which was committed to the eminent skill of a Mr. Sutherland, who performed the trust reposed in him, to the universal satisfaction of his employers; and I believe the whole was completed for something less than 3000*l*, in a very masterly and workman-like manner. Of this I think there cannot be a better proof adduced, than that it still stands *firm*, without the least apparent decay, and maintains its first position without the smallest variation: and no doubt a very trifling annual expense will keep it in its present improved state.

The author's plan to make a direct cut, and dam up the old mouth.

The author undertook it at his own risque,

and completed it.

Farther security by a pier head and jutties.

It proves to be perfectly durable,

and admit ships of five times the tonnage before admitted.

The advantages derived from it are particularly great; for ships of 250 tons burden, and even vessels of 300 tons, run in with the greatest safety at spring tides: whereas, before, those of 50 tons could not come in, but with the utmost difficulty and danger.

That

That part of Romney Marsh too, which lies contiguous, and was threatened by every boisterous tide with a total overflow, is now in safety, and the drainage of the levels is rendered complete.

Other advantages.

I beg leave now to offer to your attention a short description of the Dam, the form and materials of which may be used with success in similar situations, whether in places adjacent to the sea, or in gentlemen's fish-ponds, or rivers in the country, where weirs may be necessary for the preservation of the banks. The dam was merely formed of hay, straw, and faggots, pinned down to a *foundation* of *sand* or *silt* by short piles. I formed it as in the chart, of the shape of a double-roofed house, first putting down straw, and then over it hazel faggots, from 12 to 14 feet in length, and afterwards pinning down the whole with piles. I next filled the space between the two roofs with gravel or sea-beach, and secured this also with faggots pinned down upon it, over which resistance being precluded from its peculiar form, the influx and reflux of the tides glided so gently, that consequently every probability, not to say possibility, was annihilated of its being ever undermined or blown up.

Very easy and beneficial method of constructing the dam.

A double roof or covering of hay, &c. was pinned down upon a foundation of sand, &c.

The interstice was filled with gravel, and covered with faggots secured down.

It was also necessary that this dam should be put down in one tide, and that the mouth of the cut should be opened in the same time; for it was evident to me, that it was impossible ever to cut to sea in any other way. For unless the dam had been ready to turn the water through the cut as soon as opened, and the cut, on the other hand, ready to receive the current the moment the dam began to act, the whole work must have been entirely and unavoidably destroyed by the influx and reflux of the ensuing tide. All this I clearly foresaw: and by procuring a sufficient number of men, nearly three hundred, the business was completely finished, just as the tide touched the foot of the dam; and when it was full sea, the straw of course acted as a receiver and retainer to the *silt* brought in by the tide; which being repeated by each returning tide, the dam soon became entirely fixed, beyond a possibility of ever being destroyed; and it is now so entirely covered, that if the pier is kept in repair, the dam must ever remain unimpaired by time, and proof against the most violent floods of waters.

Difficulty that the old mouth should be closed and the new one opened in one single tide.

Successful result.

For this work, the Commissioners voted me fifty guineas (half of which I gave to my assistant) and alledged that, on account

The author's reward for his contrivance, and

attending the works: 50l.! account of the loſs of the tonnage, and the poverty of the fund, they were ſorry it was not more. This to me, under theſe circumſtances, was a ſufficient apology, and I was content. I now offer it to the conſideration of the Society of Arts, as a body in ſome degree intereſted in the proſperity of this kingdom. Should they deem what I have already received an adequate compenſation for ſuch a work, and ſuch an undertaking, at ſo inclement a ſeaſon, I am ſtill content. But if they ſhould think proper to grant me an additional remuneration, it will be received with peculiar ſatisfaction, and conſidered as a very great honour by,

Sir,

Your obedient and humble Servant,

To Charles Taylor, Eſq. DANIEL PAPE.

Cambridge, Trinity Hall,
April 2, 1803.

Reference to the Engraving of the Rev. Mr. PAPE's *Improvement of Rye Harbour, Plate* XIII. *Fig.* 1.

AA. The double roof, filled with ſtraw.

BBB. Hazel faggots, 12 to 14 feet long.

C. The ſpace betwixt the roofs filled with gravel or ſea-beach.

D. The faggots which covered the gravel ſo laid.

E. Piles of wood driven through the faggots and ſtraw into earth, at the bottom of the river, the heads of which piles are united by croſs pieces of wood.

F. The ſolid bed of the river.

G. The river at low water.

H. The high-water mark.

I. The upper ſide of the dam, which oppoſes the current of the river.

K. The lower ſide of the dam, which reſiſts the coming-in of the tide.

Fig. 2. L. Shows the place where the dam was placed.

M. The old courſe of the river repreſented by dotted lines, and which is now filled up with gravel by the tide.

N. The new canal, cut by Mr. Pape's directions, and which is now the regular channel for ſhipping.

O. The

O. The pier-head, on the east side of Mr. Pape's cut.

PP. The two jutties, on the west side of Mr. Pape's cut.

RR. The former canal, cut under the direction of Mr. Smeaton, and other able engineers; but which failed, and is since blocked up by a bank made across it, over which the present high road between Rye and Winchelsea passes.

V.

New Experiments on the Respiration of Atmospheric Air, principally with regard to the Absorption of Azote, and on the Respiration of the Gaseous Oxide of Azote. By Professor PFAFF, *of Kiel* *.

Short history of chemical researches concerning respiration.

THE great discoveries in pneumatic chemistry, the ingenious and useful applications of these discoveries to explain the phenomena of organized beings, particularly the animal economy, and the valuable researches of eminent philosophers have greatly contributed to throw light on the doctrine of the chemical effects of respiration. In consequence of these researches, phisiologists are in general agreed with regard to the most essential points of this doctrine; such as the production of carbonic acid, the use of oxigen gas, and the animal heat which results from its absorption. But the activity of philosophical enquirers has not yet succeeded in removing all the obscurities of this subject, and the disagreement between the results of various experiments relating to them, sufficiently shew, that new enquiries are requisite to ascertain the sources of such errors as still continue, and to remove them altogether. The experiments of the celebrated Davy have done much in this respect, and the researches on the nitrous oxide afford a new epocha in the chemical doctrine of respiration. The celebrated editors of the Bibliotheque Brittannique, have shewn their conviction of the great value of these researches, by the ample and instructive extract they have given, and the manner in which Berthollet in the 45th volume of the *Annales de Chemie* has given an account of the same, has sufficiently fixed the attention of philosophers on that excellent work.

Davy on nitrous oxide.

* This memoir was addressed to the French National Institute, and read at their sitting of the 25th of Messidor last (July 13.)

The

The differences which exifted in the refults of former experiments, as to the quantity of carbonic acid produced in the act of refpiration were lefs important, and might entirely depend on the conftitution of the different individuals upon whom the experiments were made; and under this point of view, a revifion of the experiments was lefs neceffary. But the part which is performed by azote gas in the act of refpiration has been too little attended to. It has been generally fuppofed to be altogether without activity. Goodwin alone thought he had obferved a confiderable abforption of azote gas; but his experiments were not made with all the neceffary accuracy, and were too directly oppofite to the experiments of Lavoifier, Seguin, Abernethy, Fothergill, Menzies, &c. to fix the attention. The experiments on the flow combuftion of phofphorus, which does not fucceed in pure oxigen gas, but is fo greatly forwarded by the prefence of the azote gas of atmofpheric air, fhew to a certain degree the advantages which this great quantity of azote gas is likely to produce in refpiration; and the unfortunately too concife refults of the laft experiments of the immortal Lavoifier on refpiration, in which it was found, that a much greater mafs of oxigen gas is decompofed in the fame time by refpiration in atmofpheric air than in oxigen gas, ftand in confirmation of the former fact. But hitherto we have poffeffed only probabilities, or refults not fufficiently connected with the fubject. To Davy it is that we are indebted for an exact and inconteftible knowledge of the active part which azote gas performs in the procefs of refpiration. But in proportion to the novelty and interefting nature of thefe refults do they require to be confirmed by the experiments of others; and it was in this point of view that I undertook laft winter a feries of experiments upon refpiration in atmofpheric air, and alfo in the gafeous oxide of azote; the principal refults of which I now venture to communicate to the National Inftitute.

Azote neceffary for the flow combuftion of phofphorus.

Experiments on the Refpiration of Atmofpheric Air, and Oxigen Gas.

Experiments of refpiration.

All the following experiments were made in the academical laboratory of the Univerfity of Kiel, which is provided with all the accurate apparatus of modern chemiftry. They were made

made for the most part in the presence of my pupils, particularly one named *Dierks*, who was most commonly the subject of experiment.

In order to determine with precision the changes which atmospheric air undergoes by respiration, and to decide respecting the absorption of azote gas, we must begin with ascertaining the diminution which a given volume of air undergoes by respiration. This first point was to be determined by accurate experiments.

Quantity of diminution undergone by air by the process of respiration.

1. The quantity of 170 duodecimal cubic inches of Paris* were respired from one of the great reservoirs of a gasometer, constructed at Paris after the model of that of Charles, over water covered with oil, to prevent the absorption of the carbonic acid gas produced by respiration. The respiration was performed once only during the time of ten or twelve seconds. The diminution was 4,72 cubic inches, or $\frac{1}{36}$ part of the first volume. This experiment being repeated twenty times in the same manner, afforded the same result.

2. 144 Cubic inches were respired once in the space of ten or twelve seconds. The diminution was four cubic inches or $\frac{1}{36}$ part of the primitive volume.

3. The same volume was respired twice during 22 seconds, and the diminution amounted to eight cubic inches, or $\frac{1}{18}$ part of the primitive volume. The same volume having been respired three times during 30 seconds, the diminution amounted to 12 cubic inches, or $\frac{1}{12}$ part of the primitive volume.

4. 60 Cubic inches were respired three times during 25 seconds, the diminution was six cubic inches, or $\frac{1}{10}$ of the primitive volume.

5. 170 Cubic inches were respired four times during one minute, and the diminution amounted to 20 cubic inches.

This experiment was several times repeated, and the diminution was almost constantly the same. Namely, 18, 19, 21, and 20 cubic inches, or $\frac{1}{27}$.

6. 168 Cubic inches respired during 50 seconds, by four great and four small respirations, suffered a diminution of 14, or $\frac{1}{12}$ of the primitive volume.

7. 480 Cubic inches by 12 respirations in 90 seconds, suffered a diminution of 24, or $\frac{1}{18}$ part.

* As these quantities are merely relative, I have not reduced them. T.

These

These results agree very well with those obtained by Davy on the diminution of air by respiration. He found the diminution by one single respiration to be $\frac{1}{43}$ part, and by respiration continued for one minute $\frac{1}{18}$ part.

The magnitude of the diminution depends not only on the time during which a given volume of air is respired, but principally on the magnitude of this volume itself; it must be proportionally less the greater the quantity of air inspired. A very essential error is seen in the results of Abernethy, who gives a greater volume to the expired than to the inspired air; and the calculations of Goodwin are founded on a mistaken basis; for he supposes the two volumes equal.

Diminution of oxigen gas by respiration.

In order to determine comparatively the diminution of oxigen gas by respiration, 170 cubic inches of oxigen gas obtained from manganese were respired in the same manner, and under the same circumstances as the 170 cubic inches of atmospheric air in the 5th paragraph. The diminution amounted to 30 cubic inches, and in other experiments, to 33, 29, 31. The mean term of which is $\frac{2}{11}$ parts of the primitive volume.

This diminution being established with accuracy, may be applied to determine the absorption of azote gas.

Experiments to determine how much azote is absorbed in the process of respiration.

8. 80 Cubic inches were respired one time slowly during ten or 12 seconds, and the air expired was received over mercury.

The relative quantity of the constituent parts of this respired air was in the centenary 4,16 carbonic acid, 16,55 oxigen gas observed by the slow combustion of phosphorus, 79,19 of azote gas. An eudiometric experiment made at the same time, gave the following proportion of the parts in atmospheric air, one carbonic acid, 21 oxigen gas, and 78 azote. The total diminution of the air was from the preceding experiments $\frac{1}{36}$. We may therefore find the true quantity of azote gas by the following proportion, 36 : 35 :: 79,19 : 76,99. If we subtract this 76,99 from 78, the primitive quantity of azote in the atmospheric air before respiration, we find a loss of 1,01 on the hundred parts of the whole mass of air breathed. But as the quantity of air inspired was really no more than 80 cubic inches, the absolute diminution or disappearance of azote gas by one respiration,

must

must be diminished in the same proportion of 100 to 80, and thus proves 0,808 cubic inches.

9. In another experiment 60 cubic inches were respired once in the time of 10 or 12 seconds, and the last portion of the expired air was received over mercury. The proportion of the constituent parts after respiration, were in the centenary 4,68 carbonic acid gas, 17,68 oxigen gas, and 77,74 azote gas. An eudiometric experiment made at the same time on the atmospheric air, gave 1 carbonic acid, 22 oxigen gas, and 77 azote gas. The true quantity of azote gas found as before, by diminishing the 77,74 $\frac{1}{36}$, is, 75,58. And this being subtracted from 77, the quantity of azote gas previous to the respiration leaves 1,42 for the azote which disappeared, supposing the respired air to be divided into 100 parts. But if we take the real number in inches, which was 60, this quantity will be expressed by 0,852 cubic inches.

10. 30 Cubic inches were respired in the same manner three times during 16 seconds. The expired air contained in the centenary 5 carbonic acid gas, 14,5 oxigen gas, and 80,5 azote gas. The atmospheric air contained by experiment at the same time, 1 carbonic acid gas, 29,75 oxigen gas, and 80,025 azote gas. This by the same process of computation gives a diminution of 4,235 in the 100, or in cubic inches 1,2705.

Remarks on uses, &c. of azote.

These experiments which were several times repeated, and constantly with the same result, decisively shew that azote gas is absorbed in the act of respiration, and the active part it performs. Hence we may more easily understand, why azote gas compared with other mephitic gases is so little noxious to our lungs; so that according to the experiments of Lavoisier and Seguin, animals live very well in a mixture of 15 parts azote gas, and one part oxigen gas; whereas the same animals were suddenly suffocated in a mixture of 40 parts oxigen gas, 45 azote gas, and 15 carbonic acid gas. Hence we may comprehend, at least to a certain extent, the extraordinary effects of the gaseous oxide of azote; we may form some notion of the transformation of the chyle, which is less anamalized or azotized in the lymphatic part of the blood, but becomes more so in the act of respiration. But the quantity of azote gas absorbed by one single respiration is not very considerable, which

which agrees perfectly with the experiments of Davy, who found that no more than 5,1 cubic inches of azote gas were absorbed by 19 respirations of a volume of 161 cubic inches.

Experiments shewing the quantities of carbonic acid produced in respiration.

11. To determine the quantity of carbonic acid gas produced by the respiration of atmospheric air, 60 cubic inches were respired once during ten or twelve seconds, and received over mercury when expired. Lime water absorbed 4,68 parts in 100. This experiment being several times repeated gave the same result. The last portion of expired air being several times transferred through lime water was diminished 4,9 parts in 100.

12. 20 Cubic inches respired three successive times during 10 seconds afforded no more than five hundredths of carbonic acid gas.

13. 170 Cubic inches were respired four times during 50 seconds, the quantity of carbonic acid gas obtained was 5,8 hundredths.

14. 170 Cubic inches were respired from a bladder eight times in one minute. Lime water absorbed 8,2 hundredths.

This quantity of carbonic acid produced by respiration, afforded a term of comparison to ascertain the quantity of the decomposition of oxigen gas in respiration from the same quantities of atmospheric air, and of pure oxigen gas.

Oxigen gas produces more carbonic acid in respiration than atmospheric air does.

The preceding experiments (7) had shewn that the diminution of oxigen gas was more considerable than that of atmospheric air. From this fact it might be expected, that the production of carbonic acid gas would likewise be more considerable; and this was confirmed by direct experiments.

15. 170 Cubic inches of oxigen gas obtained from manganese, were respired four times during 50 seconds; the diminution was 30 cubic inches. The quantity of carbonic acid produced was 8,2 hundredths. Atmospheric air respired in the same manner, and under the same circumstances, gave only 5,8 carbonic acid.

16. 70 Cubic inches respired from a bladder during 50 seconds, also gave eight hundredths of carbonic acid.

Experiments on the Respiration of the Gaseous Oxide of Carbon.

Observations on the method of obtaining gaseous oxide of azote.

The gaseous oxide of azote was obtained by the process of Davy from crystallized nitrate of ammonia. This nitrate of ammonia affords very different products in different temperatures.

peratures. I have made a confiderable feries of experiments on this fubject, which I fhall fhortly fubmit to the National Inftitute. I fhall only remark in this place, that the oxigenated muriatic acid is obtained at the commencement, if the nitrate of ammonia be not entirely free from muriatic acid; that at a temperature not exceeding 220 degrees of the centigrade thermometer, the gafeous oxide of azote is obtained in great quantity, and very pure, without any mixture of thofe white vapours which have the tafte of muftard; but that a temperature ftill higher, efpecially at a red heat, the gafeous oxide of azote is no longer difengaged but nitrous gas is formed, and very peculiar white vapours which I am at prefent examining. To prevent any explofion, I always mix the nitrate of ammonia with very pure fand. To obtain the gafeous oxide of azote in a very pure ftate, the diftillation muft be made on a fand bath, and the fire carefully managed. When every thing fucceeds properly, the gas is fo pure, that it may be refpired immediately; it has an agreeable tafte, almoft facchar vinous. If it be mixed with the white vapours produced by too ftrong a heat, time muft be allowed for them to be depofited. The effects which Davy has obferved, and Pictet has defcribed with fo much intereft in his fecond letter in the 17th Volume of the *Bibliotheque Britannique*, were perfectly confirmed in my experiments. Several perfons who refpired this gas were exalted abfolutely in the fame manner. One of thofe who refpired it was very fpeedily intoxicated, and put into a very extraordinary and moft agreeable extacy. Others refifted fomewhat longer; one only feemed to be fcarcely at all affected. The exaltation always paffed over without leaving any perceptible relaxation. I ftill continue thefe experiments. Perhaps this gas may become a powerful remedy for melancholy affections. I fhall not fail to communicate the refults of my experiments to the National Inftitute.

Davy's experiments fucceeded perfectly with our author.

VI. *Experiments*

VI.

Experiments on Gum Arabic and Gum Adracanth.
By M. VAUQUELIN *.

Red gum adracanth left after combustion $3\frac{1}{2}$ hundredths chiefly carbonate of lime, with a little iron and phosphate of lime.

TEN grams of red gum adracanth produced on combustion three decigrams and a half of white ashes. These ashes dissolved in muriatic acid with effervescence, and gave forth an odour of sulphurated hidrogen. Their solution deposited a precipitate by ammonia, which was phosphate of lime and oxide of iron. The oxalate of ammonia precipitated from it much lime. Thus red gum adracanth contains in 100 parts about $3\frac{1}{2}$ of ashes, which was composed for the most part of carbonate of lime, a small quantity of iron, of phosphate of lime, and perhaps of a very minute portion of alkali.

White gum adracanth left a residue of 3, which contained the same principles and alkali.

2. Ten grains of white gum adracanth submitted to the same proofs, gave three decigrams of ashes, which were composed of the same principles as the red kind, with the addition of a little potash.

Gum arabic left 3 containing no alkali.

3. Ten grains of gum arabic burnt as the others, left three decigrams of ashes, which were composed of the same elements as the preceding, except that they gave no sign of the presence of alkali or of sulphur.

Opacity and difficult solubility of gum adracanth.

I formerly thought that the opacity of gum adracanth, and the difficulty of its solution in water, might be occasioned by a greater proportion of earthy matter; but after these experiments it appears, that they are due to another cause.

The lime in gums is combined with acid, forming a soluble salt.

The lime which I found in the gum, which I am about to mention, was doubtless neither in the state of carbonate, and still less in that of quicklime; for the solutions of the gum were not in the least alkaline, but on the contrary, slightly acid; at least a bit of the gum rubbed on some paper well moistened (with blue vegetable juice) made it sensibly red. It is also certain, that oxalate of ammonia and carbonate of potash occasion precipitates in the solution of gum arabic, and that acetite of lead does not form any. It follows from this, that the lime is most probably united to some acid in the gums, which doubtless is a vegetable acid; for in being decomposed they leave their bases combined with carbonic

* Annales de Chimie, Tom. 54.

acid;

acid; but can be neither the oxalic, the tartarous, or the citric, because their combinations are insoluble in water, and that besides they exist but in a small number of vegetables; still less can it be the benzoic, the gallic, the *moroxalic*, or the *honestic*, which are very rarely found in naturally, and of which the three last also form insoluble compounds.

There only remains to decide between the acetous and the malic acids, which are the most abundant in the vegetable kingdom. The first forms, as is well known, soluble combinations with all the substances with which they are capable of union; some of them are even deliquescent. It is besides the most frequent result of the operations of nature in the vegetable and animal systems, since it is formed by vegetation, by fermentation, the action of the more powerful acids, and by the influence of heat.

The acid must be either the acetous or the malic.

The combinations of the second are for the most part insoluble in water; that which it forms with lime particularly, is not sensibly soluble, but when there is an excess of acid; and its existence in nature is by no means so frequent as that of the acetous acid; and as the lime which is found in the transparent gums has been incontestibly dissolved in the juices of the vegetables which produce these substances, it is much more probable, that this earth is in them combined with acetous acid than with any other.

But the malic forms insoluble compounds with lime, consequently the acid is more likely acetous.

It is also probable, that the small quantity of potash which I found in the ashes of the burnt gums, is united to the same acid, which explains why these substances are so sensible to humidity, and soften so much as to prevent their pulverization.

The potash in gums also forms an acetite.

I am, however, much inclined to think, that in certain opaque adracanth gums, which are of difficult solution, and yield much lime on incineration, this earth is combined with malic acid. I have had occasion lately to examine a gum gathered by M. Palissot Bauvois, from the cochineal nopal, which was opaque, swelled with water, did not dissolve uniformly, and which yielded eight per cent. of lime. And as the sap of every cactus which I have analyzed, yielded more or less acidulous malate of lime, there is great reason to believe, that the species of it which nourishes the cochineal contains it likewise: and that it is the presence of this salt proceeding from the vegetable, and dissolved in the sap along

Some gums contain lime in greater proportions.

General results. with the gum, which causes its opacity, and obstructs its solution in water. It results at least from these experiments, that the gums contain, first a calcareous salt, most commonly the acetate of lime; secondly, sometimes a malate of lime with an excess of acid; thirdly, phosphate of lime; fourthly and lastly, some iron which is probably also united to phosphoric acid.

VII.

Method of obtaining Cobalt pure. By M. TROMSDORF *.

Zaffre detonated with charcoal and nitre.

FOUR parts of zaffre well pulverized are to be mixed carefully with one part of nitre, and half a part of charcoal in powder: this mixture is to be projected in small quantities into a red hot crucible, and this operation repeated three times, adding each time to the residue new portions of the nitre and the charcoal.

Fusion with black flux.

The mass resulting from these detonations ought to be mixed with one part of black flux, and exposed during an hour in a crucible to a red heat.

The metallic button again detonated.

The whole is then to be left to cool; the metallic cobalt to be separated, pulverized, mixed with three times its weight of nitre, and the mixture detonated with the precautions above mentioned.

Lixiviation separates the acid and arsenic.

The iron contained in the cobalt will thus become strongly oxidated, and the arsenic acidified combines with the potash: The mass pulverized is to be lixiviated many times, and repeatedly filtered; and thus the arseniate of potash formed will be separated from the insoluble residue that contains the cobalt.

Nitric acid dissolves the cobalt alone.

This residue is then to be treated with nitric acid, which dissolves the cobalt without attacking the iron which is found oxidated to its *maximum* of oxidation,

Evaporate and redissolve.

The solution is then to be evaporated to dryness, the residue redissolved in nitrous acid, and the liquor filtered, to separate the last portions of the oxide of iron, which might have escaped in the first operation.

* From the Annals de Chimie, Tom. 65.

All

All that remains to be done after this is, to decompose the nitrate of cobalt by potash, to wash the precipitate, and to effect its reduction by means of heat.

Precipitate the cobalt by potash and reduce.

VIII.

A new Method of extracting Sugar from Beet-Root. By M. Achard *.

THE roots of the beet, after being properly cleansed, are sliced and pressed. The juice obtained is thick, and of a deep colour: it contains, besides sugar, albumen, fecula, and other matters from which it must be cleared, in order to obtain the pure sugar. In this process of separation it is, that the art of procuring sugar from the beet-root consists.

Beet roots are sliced, pressed, and the juice separated.

In a boiler of tin, or of tinned copper, mix 100 lbs. of the juice of beet root with $3\frac{1}{4}$ ounces of sulphuric acid diluted with one pound of water; then pour it off, and let it stand for 12, 18, or 24 hours; 12 hours are sufficient, but 24 will not be detrimental to the process, as the acid prevents any change in the juice. In order to separate the sulphuric acid, put into the liquor $7\frac{1}{2}$ ounces of wood ashes, to which add soon afterwards, 2 ounces or $6\frac{1}{2}$ drachms of lime slaked in water. The sulphuric acid coagulates the albumen, the wood ashes, consisting chiefly of lime, and the lime itself separate in their turn the acid, in form of an almost insoluble salt. It will here be recollected, that in the West Indies, in the fabrication of coarse sugar, and in the refining houses of Europe, lime is used to assist the separation, and the crystallization of this article.

One 26th part of sulphuric acid is added to the juice, and after standing 1-12th of wood ashes, and one part of lime. The albumen, &c. are thus separated.

After this first operation the beet root must be clarified; for which purpose it must be poured into a boiler so placed as that the fire may act equally upon all the whole surface of the bottom, in which it is to be heated to a state bordering upon ebullition, but must not be suffered actually to boil. After drawing out the fire, the syrup is to be skimmed till the skum arises in blackish flakes. The liquor is now to be filtered through flannel, which must be done with caution, lest the dregs pass through with the syrup. The skum and the dregs are good for fattening swine.

Farther purification by boiling, scumming, and straining.

* Van Mons's Journal, Vol. VI.

The

Brisk evaporation.

The syrup, thus clarified and filtered, is placed in a shallow cauldron, to the depth of not more than six inches, and evaporated over a brisk fire, whereby it is prevented from becoming a liquid saccharine mucus, which resists all attempts to crystallize it.

Cooling and farther purification by subsidence.

When reduced to about one half of its quantity, the syrup is to be poured into tin vessels about six feet in height, and half a foot in diameter, with cocks about six inches from the bottom. It must here be left for two or three days, during which time it precipitates whatever remaining impurities it may contain, particularly gypsum. At the end of this period, the liquid may be drawn clear off, and replaced in the shallow boiler, but only to the height of three inches, to evaporate; the fire to be gradually augmented, as the syrup thickens, until it be in a state of ebullition. The fire is then to be damped to prevent the sugar from burning, which would render it unfit for crystallization.

Evaporation till the syrup draws a thread.

When the syrup becomes fibrous, the fire is to be extinguished.

Crystallization or graining in a warm apartment.

In about half an hour afterwards, the syrup is to be poured into cones, of which the mouths are stopped with linen cloths, and containing a little coarse sugar-candy, grossly pounded. These cones are set in a room whose temperature is from 10° to 20° of Reaumur.

When the several operations have been dexterously managed, the sugar will crystallize in 24 hours: but if the evaporation, or baking, has been too hasty, the whole becomes a granulous mass, with the interstices filled with melasses.

The melasses drawn off

When the sugar is well crystallized, the mouth of the cone is to be opened, and an earthern vessel placed under to catch the melasses: this operation, according as the syrup has been more or less baked takes three or four weeks.

leave coarse sugar.

The substance remaining in the cones, of a yellow colour, more or less tinged with white as the baking has been well or ill conducted, in granulated crystals of various sizes, is the coarse sugar of beet-root.

Improvement.

Mr. Achard, in order to save time, and to avoid the use of vessels for settling the liquor, afterwards deviated from his original plan, by adding to the syrup, when half evaporated as above described, for every twelve quintals of roots used, five

five quarts of skimmed milk, and shortly afterwards one quart of vinegar. He then proceeded with the second evaporation in the boiler.

This sugar by refining may be made to answer all the uses of that of the West Indies, and may be rendered equally white by the usual process. Subsequent refining as usual.

IX.

On Nickeline (Niccolanum), *a Metal in many Respects resembling Nickel, lately discovered by Dr.* J. B. RICHTER.*

I HAVE long since conjectured in analysing the cobalt ores of Saxony, that they contained, besides cobalt, arsenic, copper, nickel, and iron, another metal which resembled nickel in many of its properties, but the means which I have hitherto employed to separate it did not before afford me any satisfactory results. Suspicion of a new metal in cobalt.

I was chiefly surprised that nickel, after being purified by the liquid process from cobalt, iron, and arsenic, and after that reduced without the addition of a combustible body, never formed a mass, but was always found dispersed in small particles in a hard heavy mass, which had the appearance of the remains from vitrified copper, Remarkable fact, that nickel cleared of iron and arsenic and fused, is almost dispersed in small globules through a hard, heavy, blackish-grey mass.

This hard matter had no metallic lustre, neither was it attracted by the magnet: Its colour was of a blackish grey on the surface, with a small degree of brightness; and in powder it was brown, greyish, and greenish.

Some weeks ago I endeavoured to reduce *per se* almost half a pound of oxide of nickel, which I had purified as well as possible by the liquid process, for the greatest part of a year, at a considerable expence: as this oxide was not of a lively green, I though this was caused by the "extractive matter" which might be in the potash employed for the precipitation of the sulphate of nickel from the ammoniacal preparation: it is true that this triple combination had not that beautiful grass-green colour which it commonly had; but I thought this might Experiment with a large quantity of the oxide of nickel.

* *Annales de Chimie*, LXIV.

might be caused by the substitution of the potash to the ammonia mixed with the copper, which could not be separated but by the reduction *per se*.

Only a small quantity of nickel in a mass was obtained.

From these ideas I hoped to have at least four ounces of perfectly pure nickel, but was disagreeably surprised by finding in the crucibles, which were deformed in the usual manner, and perforated by the vitrified copper, a rough mass with the appearance I have before mentioned, and which contained only a morsel of about two and a half drams, and consequently only five drams of pure nickel in the two crucibles. I reduced to powder in an iron mortar the remaining mass (which could not properly be called scoriæ), and separated from it by the sieve and the magnet, the particles of nickel which it might contain, which produced near two and a half drams more; and that nothing might be lost, I treated the powder with nitric acid, which attacked it vigorously at the first, and gave a solution of nickel, but after that did not act on it in the least, so that the powder was but little diminished in weight: in exposing this matter to reduction *per se*, it produced no regulus, but merely agglutinated its parts.

The dense accompanying matter was then pulverized and the nickel separated by the magnet and by nitric acid. Strong heat applied to the mass gave no more regulus.

The mass being again powdered was urged with charcoal, and afforded more than half its weight of metal in one mass.

Having again pulverized the mass, which weighed almost $4\frac{1}{2}$ ounces, I mixed with it one ounce of charcoal in powder, and exposed to the fire of a porcelain furnace during eighteen hours, in a crucible closed with a luted cover, in a part of the furnace which seemed to me to have most heat. After having broken the crucible, which was in a sound state, I found, under a scoria of a deep blackish-brown colour, a well fused button of metal which weighed two ounces and three quarters: it was not at all connected with the adjoining parts of the scoria, and had at its inferior part a particular shape, which was caused by cavities which were not produced by the crucible.

It was steel colour, rather hard, scarcely malleable, magnetical, &c.

This metal had the grey colour of steel, inclining a little to red: it presented in its fracture a grain not very fine: it was rather hard: could be extended a little under the hammer in a cold state: heated to redness it endured little the strokes of the hammer: it was attracted by the magnet, but not so strongly as either iron or nickel: *it had many properties common to nickel, but it was distinguished from it entirely by others.* As many of these properties were such, that those

not

not well acquainted with nickel in its perfectly pure state might take it for that metal, I have called it Nickeline (*Niccolanum.*) Name Nickeline.

The nickeline was free from all the metals which are found in the cobalt ores, except a little copper.

The specific gravity of cast nickeline, which enters more readily into fusion than nickel, is 8,55; and of forged nickeline 8,60. On putting it into nitric acid and heating it, it is attacked more quickly than nickel: I remember having observed an equally violent action of nitric acid on nickel reduced by charcoal, which I then considered as pure, and which I dissolved in order to precipitate from it by potash an oxide, which I might reduce *per se*. Specific gravity 8,6. Nitric acid dissolves it.

The solution of the nickeline went on well; being come to the point of saturation, it had a blackish-green colour, and assumed a gelatinous consistence.

I employed my first care to separate from it a part of the iron which I thought it contained, and left it to dry a little over a spirit lamp: the mass became continually of a deeper green, and in approaching to dryness it gave out much red vapours, and the residue became of a blackish grey; I added distilled water to it, which dissolved but little of it, and that which was dissolved was an insignificant quantity of nickel. Separation by drying of the acid. Residue a black powder.

I poured muriatic acid on the blackish powder well washed, which gave a green solution, in disengaging a strong odour of oxigenated muriatic acid. Soluble in muriatic acid.

The muriatic solution was, as well as the nitric solution, of a deep blackish grass-green colour: being evaporated to dryness, it produced a reddish mass, which became green in a moist air, and which communicated the green colour to water in which it was dissolved. Green solution; which when dried gave a reddish mass that turns green by damp.

This dark-coloured oxide of nickeline was insoluble in nitric acid, and in sulphuric acid; but if sugar or alcohol was added, the solution took place with facility at the boiling point. Dark oxide of nickeline not soluble in nitric or sulphuric acids without combustible matter.

The sulphate of nickeline, being combined with water, is also of a blackish green; but it assumes a pale red colour on being deprived of the water. Sulphate of nickeline.

If carbonate of potash be added to the preceding solutions of nickeline, it occasions a precipitate of blue carbonate of nickeline, inclining a little to grey and green, and of a pale tint; This combination is very light and soft, and dissolves in Precipitate by carbonate of potash;

the

the acids with a strong effervescence. I remember to have had, some years ago, this precipitate of a bad colour, and not then to have examined it, considering it as a mixture of iron, nickel, and arsenic, (which last continually made itself noticed by its odour of garlic): But at last I suspected its nature.

by caustic potash;

If the solution of nickeline is decomposed by caustic potash, it gives a precipitate which resembles in its colour carbonate of chrome; that is to say, it is of a deep greenish-blue, which does not change when it is washed: being dried with a gentle heat, it assumes a pale colour, which becomes deeper when it is moistened with water.

by ammonia.

If any of the foregoing solutions of nickeline is mixed with ammonia to excess, the liquor assumes a pomegranate red colour, and remains transparent; which proves that it does not contain any iron, beause that this latter is not soluble in ammonia. By candle-light this solution is with difficulty distinguished from that of perfectly pure nickel; but by daylight, this latter is of an amethyst red colour, as I have elsewhere remarked.

Points of comparison between nickeline and nickel or cobalt.

I shall now compare the principal properties in which nickeline resembles altogether, or in part, nickel or cobalt, and those in which it is distinct from them.

It resembles cobalt—

Resemblances of nickeline and cobalt.

1. By its property of super-saturating itself with oxigen at the expence of the nitric acid, and thus forming a body which resembles the black oxide of manganese with regard to its solubility in the acids: 2. By its property of not being reducible but by the intervention of a combustible body.

It differs from cobalt—

Differences between nickeline and cobalt.

1. By the blackish-green colour of its solutions, even when they are entirely neutralized. It is known that the neutral solutions of cobalt in the sulphuric, nitric, and muriatic acids, are of a crimson-red colour; and that the muriate of cobalt alone becomes of a greenish-blue on being deprived of its water: from whence it happens that an excess of acid produces this colour, because it combines with the water: With the muriate of nickeline precisely the reverse takes place; when mixed with water it is green (although of a less beautiful colour than the cobalt without water), and when deprived of its water it becomes reddish.—2. By the colour of its

its carbonate: that of cobalt is of a beautiful poppy-blue, but the carbonate of nickeline is a bluish-green inclining to a pale grey.—3. By the colour of its oxide precipitated without carbonic acid: that of cobalt is of a deep blue, and changes on washing to a blackish-brown; but this oxide of nickeline is of a greenish-blue, and its colour does not change.

Nickeline resembles nickel—

Resemblances between nickeline and nickel.

1. By its strong magnetic quality; although this is not so great as that of nickel.—2. By its malleability, which however is less than that of nickel.—3. By the deep green of its solutions; although this colour is not so beautiful as that of the solutions of nickel.—4. By the loss of this green colour when its neutral combinations are deprived of water.—5. By the colour of the acid solution with an excess of ammonia, which cannot be well perceived by candle-light.

Nickeline differs very distinctly from nickel—

Differences between nickeline and nickel.

1. Because it cannot be reduced without a combustible body added to it.—2. Because nitric acid attacks and oxidates it more easily. Nickel is not near so readily acted on by the nitric acid if it is not mixed with the nickeline, which almost always happens with the magnetic nickel which is considered to be in a state of purity, and which has not been reduced *per se* before my discovery.—3. It also differs from nickel by the property first mentioned of those in which it resembles cobalt.—4. By the colour of its combinations with the acids, when deprived of water: This colour in nickel is almost a buff *(chamois)*, and in nickeline a reddish, except in the nitrate of nickeline, which cannot be deprived of water without decomposing it.—5. By the colour of the precipitates, mentioned in the second and third articles concerning the properties wherein this new metal differs from cobalt, which are in those of nickel of a green colour entirely different from those of nickeline, which latter are of a much more agreeable green, especially those of the carbonate.

Letter

X.

Letter from G. CUMBERLAND, *Esq. on a Project for extended Roads on the Principle of the inclined Plane.*

To Mr. NICHOLSON.

SIR, *Oct.* 26, 1805. *Weston-supra-Mare.*

Account of rail-roads,

ABOUT ten years ago having frequent occasion to remark, and suffer from, the miserable state of the roads from Staines to London during the winter season, I ventured to propose (not having at that time either seen or heard of rail-roads) a plan which I called a *truck-road* for the whole of that stage, because it was intended to convey all sorts of goods and even carriages on trucks, going to town on one side of the old road and returning by the other.

communicated to Dr. Anderson.

This plan I sent some time after to Dr. Anderson, with a drawing, for his *Recreations*, but by some accident it was mislaid and lost; and the reward of my trouble was the sly sneers of my grave Windsor neighbours, to whom it was known, accompanied with a sort of pity for heads capable of proposing such eccentric inventions.

Time however revenged my cause, by showing them the practicability of such schemes in the progress of the Surry undertaking.

Another plan by an inclined road.

At the same time *another* plan of expeditious conveyance occurred to my mind, but which I was deterred from then producing owing to the cold reception my first contrivance met with—And as no one, as far as I can learn, has hitherto brought forward any improvement of the kind (although so very obvious that it might easily be suggested to the mind of a child who had heard of roads on inclined planes,) I take the liberty to recommend it to your patriotic publication, convinced that whatever may, at one time or other, be of service to mankind, will be always sure of a favourable reception at your hands.

Particular detail. Dispatches may be rolled in a spherical case down a long inclined channel.

The plan I propose then is this:—That all dispatches and post-letters may, wherever it is compatible with the inclination of the ground, be conveyed ten or fifteen miles to and from London by means of *iron or wooden shells of a globular form,* rolling

rolling in a cylinder of brick or stone. When closed and locked, a due momentum being given at a proper elevation, it is easy to see that their speed and security must far surpass any other mode of conveyance that we at present know of; and all that would be necessary in addition to the machine would be to have proper beds of sand or wool bags to blunt their projectile force at the end of their career.

I shall not at present enter into the discussion of the construction of the tube-road, or go to a calculation of their expence; but if you think the bare hint worth publishing it will give me pleasure, should the idea be approved, to go into all the minutiæ of their utility in other respects, and the means of their ultimate accomplishment;

Being always, Sir,

Your obliged humble servant,

G. C.

XI.

On the State of Provincial Societies for Scientific and Literary Improvement. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Great advantage that would result to the town of Aberdeen from the establishment of a public library, &c.

BEING in the custom of visiting Aberdeen, in one of my last tours, I inquired if there were any Antiquarian or Literary Society, or Subscription Library there, and was much surprized to find neither one nor the other; there is, I was told, an Athæneum, in which a good number of newspapers, and some of the most respectable periodical publications, are taken in, and in a room above that, a circulating library; this last I knew to be the property of two very respectable booksellers in Aberdeen, and I believe the former is also, but the two united by no means effect the utility of either a literary society, or a *subscription* library, *in which the books, &c. are the property of the members, and whose concerns, such as choosing and ordering books, and the like, are conducted by a committee, chosen out of the subscribers.* Few of those who know that there is no such institution there, when they consider the respectability of the place, either in a commercial or literary view,

view, but must feel greatly astonished; and more particularly will the want appear, when it is also known, that in Montrose, Arbroath, Dundee, and Perth, places much smaller than Aberdeen, and not possessing any college establishment, there are *subscription* libraries, *on the above plan*; nay, that Perth hath also, an Antiquarian Society! Subjoined is a list of some other places in North Britain, enjoying the advantages of such establishments as I would recommend to Aberdeen; some of whom, it is obvious, have not near the prospect of success that that place could command.

Glasgow, Paisley, Greenock,
Kilmarnock, Linlithgow, Haddington.
On the borders of Northumberland, Dunse and Kelso,

and to other respectable and opulent places.

The annual subscription to none *of these is more*, in some cases *not so much*, as to the Athæneum of Aberdeen and others, and they all possess very excellent and increasing selections of books.

I am, Sir,

Your's respectfully,

A TRAVELLER.

York Hotel, Bridge Street,
Black Friars.

P. S. I am sorry to be informed that neither Inverness, Banff, or Peterhead, possess such institutions, particularly the first, which presents such an abundant number of objects to the antiquarian, and is surrounded by, and contains, so many gentlemen of distinguished liberality, and ingenuity; at this place the northern meeting was established for the avowed purpose o promoting intercourse amongst distant families, but how much more might be effected of general amelioration and comfort, by the establishment of a Literary and Antiquarian Society, in which subjects connected with general improvement might be discussed, and books in chemistry, agriculture, and other more immediately useful parts of knowledge, collected.

Notice

XII.

Notice of certain Inſtances of waſteful Negligence in ſome Fiſheries in the North. By an ENQUIRER.

To Mr. NICHOLSON.

SIR, *London, Oct.* 10, 1805.

IT is mentioned in the Statiſtical Reports of Banff and Peterhead, that the fiſhermen there never think of carrying their fiſh along the coaſt ſouthward, which they might do, to Leith, in 24 hours, or with a good briſk wind to Berwick-upon-Tweed, or even Newcaſtle-upon-Tyne: but when their reſpective towns are ſupplied *they throw the remainder upon the dunghills for manure!* this was poſitively affirmed to me as a truth, by a gentleman of great reſpectability of Aberdeen.—At Arbroath another cuſtom equally as extravagant in its kind prevails, and of which I have been a witneſs: the crab fiſhery there is ſo productive, that after boiling them, *the bodies of the crabs are thrown away*, and the *large claws only* brought to table! Ought not ſuch amazing waſte to be remedied? * Inſtances of waſteful negligence in ſome fiſheries.

Your's reſpectfully,
An ENQUIRER.

XIII.

On Bile. By M. THENARD †.

BILE has been commonly conſidered as a ſaponaceous liquor charged with albumen: but it has been found, upon cloſer inveſtigation, to preſent phenomena which cannot be accounted Bile conſidered as a ſoap with albumen.

* *Qu.* What may the value of manure procured from fiſh at theſe places, compared with the price of the article at the neighbouring markets, ſubject to the deduction of carriage (coaſtwiſe), and the effect of a rival ſupply from nearer parts of the coaſt?—The facts which would ſolve this queſtion, would ſhew whether the fiſhermen neglect their intereſts in theſe proceedings.—W. N.

† Bulletin des Sciences, No. 95.

for

for merely by the presence of these principles: this is more particularly to be observed on submitting it to the action of fire and of acids.

Destructive distillation leaves one-eighth, in which is only one-fifth of soda; and this cannot saponify the oil.

Bile, if distilled to dryness, leaves a residuum equal to $\frac{1}{8}$th of its original weight; from 100 parts of which calcined is obtained a carbonaceous matter, comprising several kinds of salt; as marine salt, phosphate of soda, sulphate of soda, phosphate of lime, oxide of iron, and four parts of soda. Bile therefore contains no more than $\frac{2}{100}$ parts of its weight of soda.

So small a portion of alkali would not be sufficient of itself to dissolve that quantity of oil which is known to exist in bile: a fair presumption may therefore be entertained that this liquor contains some other property to supply the absence of alkali. This conjecture increases to strong probability, if not to absolute certainty, in attending to the action of acids on bile.

Acid separates oil and albumen from bile; the clear fluid is bitter, and affords by evap. a residuum.

If a few drops of acid be mixed with bile, a liquor of a reddish tint is obtained, which stains paper of a bright yellow. In this experiment little or no precipitation is perceived; but on the addition of more acid, it takes place abundantly: the matter deposited consists of albumen joined with a very small portion of oil, not at all correspondent to the quantity of these substances to be found united in pure bile. The liquor remaining after filtration is of an extremely bitter taste, and leaves on evaporation a residuum equal to what is obtained from a like quantity of bile in its original state.

The oil with an alkali and albumen is not bile.

On dissolving the oil, which had been previously obtained from bile, in alkali, and adding to the ley produced, a portion of albumen, a combination took place which was decomposed by the most feeble acids, and from which vinegar precipitated all the oil. This combination, therefore, was not bile; consequently bile consists not merely of albumen, oil, and soda; and this is the reason why soluble salts, barites, strontian, lime, and several metallic dissolvents, make no impression upon bile.

Bile contains a peculiar matter.

No longer doubting that there existed in bile a matter peculiar to itself, I endeavoured to separate it; and after a few trials, I succeeded, by means of a combination of acetous acid with lead.

Acetite of lead precip. the oil and alb. The liquor by evap.

On pouring into bile acetite with a slight excess of oxide of lead (that is, acetite of common lead boiled with about the 6th part of its own weight of litharge deprived of its carbonic acid)

acid) the whole of the albumen and oil were precipitated; the liquor being filtered, the oxide of lead and acetite were separated from it by means of sulphurated hidrogen; and by evaporation, after having again filtered the liquor, a substance was obtained whose flavour was at once saccharine and acrid, somewhat similar to the juice of certain kinds of liquorice. But as this substance was still supposed to be charged with the salts of the bile, changed into acetite, by the acetite of lead, it was precipitated with acetite super-saturated with oxide of lead, containing one part of the quantity of acid found in common lead; the precipitate was dissolved in vinegar, to free it from the sulphurated hidrogen, filtered, and again evaporated; by which means the matter was obtained in its greatest purity.

gave a substance which when pure

Its principal qualities are:

had the peculiar qualities here enumerated.

1. Being soluble in water, and in alcohol, slightly deliquescent.

2. It is not precipitated by acetite of common lead; but is entirely so by the saturated acetite of lead, which precipitate is soluble in acetite of soda.

3. It will not ferment with yeast; will give no ammonia by distillation; and is not affected by the presence of nutgall.

4. It dissolves the oily matter of bile: but to facilitate this operation, it is necessary to dissolve the two matters together in alcohol, evaporate, and wash the residuum in water. One part of the saccharine and acrid substance dissolves only three-fourths of the oily matter. Now, as these matters are nearly in equal proportions in bile itself, it must be admitted that soda contributes towards the complete dissolution of the oil; nevertheless acids scarcely, if at all, affect it.

In reflecting on the above experiment and its results, I concluded that bile was a triple compound of a little soda and much oily and saccharine matter; that acids decomposed it but in part; in other words, that it was capable of containing an excess of acid without having its portion of soda neutralized. I therefore calcined bile that had been acidulated with sulphuric, muriatic, and other acids, and found in each case the soda left in the calx: it is therefore very probable that the saccharine matter, in conjunction with the oil, decomposed a certain quantity of marine salt, and destroyed the power of the acid.

Bile consists of a little soda and much oily and saccharine matter, &c.

Determination of component parts or bile.

It would have been of little service to describe the constituent parts of bile, had their proportions been left unascertained; I have therefore endeavoured to determine them in the following analysis:

Analysis.

By means of nitric acid, I separated the animal substance, which is supposed to be albumen, with a very small portion of oil: this being soluble in alcohol and the other not, it was easy to ascertain the weight of each. I then precipitated all the oily matter, with acetite and a small excess of oxide of lead: this precipitate being mixed with the metallic oxide, I dissolved it in weak nitric acid; after filtering the liquor, I deprived it of the lead which remained, by means of sulphurated hydrogen; and by evaporation, I obtained the peculiar substance, mixed, indeed with the salts of the bile, which had mostly undergone a change by the acetite of lead, and whose weight had noted.

I ascertained the quantity of soda by calcining 100 parts extracted from bile, and comparing with much care how much the residuum would imbibe of acid at 16°, with the quantity imbibed by pure soda. I also, by means not necessary to state here, obtained the quantity of each of the other salts contained in bile; from all which experiments, made with the utmost care, I conclude that 800 parts of the bile of an ox contain—

Numerical result.

Water	700 Parts
Oily matter	43
Particular substance	41
Animal substance	4
Soda	4
Marine salt	3.2
Sulphate of soda	0.8
Phosphate of soda	2
Phosphate of lime	1.2
Oxide of iron	0.5
	799.7

N. B. This calculation is $\frac{3}{10}$ deficient of the given q

Bile forms an interesting subject for a number of other researches: the varieties to be found in the several species of animals, and which a multitude of circumstances, particularly a morbid affection of the organ which secretes it, may modify;

the calculi which are there formed, and are of a peculiar nature; the oleaginous and animal substances; and that particular matter, differing from all others hitherto known; will not fail to excite a lively interest, and are the subject of several other Papers which I purpose shortly to bring before the public.

XIV.

Quotation from Sir GEORGE STAUNTON'S *Embassy, containing a Description of Fire Works unknown in Europe. Proposed by a Correspondent with a View to obtain Explanation of the Means by which they were produced.*

To Mr. NICHOLSON.

SIR,

I presume to think it will accord with the general aim of your excellent collection to insert the following quotation; and I indulge the hope that your compliance with my request for that purpose may produce an explanation from some of your ingenious correspondents. Introduction.

I am Sir,

Your constant reader,

P. M.

"After the ballets, Fire-works were played off; and even in the day-time had a striking effect. Some of the contrivances were new to the English spectators. Out of a large box, among other instances, lifted up to a considerable height, and the bottom falling out as if it were by accident, came down a multitude of paper lanterns, folded flat as they issued from the box, but unfolding themselves from one another by degrees. As each lantern assumed a regular form, a light was suddenly perceived of a beautifully coloured flame, burning brightly within it; leaving doubtful by what delusion of the sight those lanterns appeared, or by what property of combustible materials they became thus lighted, without any communication from the outside to produce the flame within. This devolution and developement were several times repeated, with a difference of Remarkable fire-works of the Chinese.

of figure every time, as well as of the colours, with which the Chinefe feem to have the art of clothing fire at pleafure. On each fide of the large box, was a correfpondence of fmaller boxes, which opened in like manner, and let down a kind of net work of fire, with divifions of various forms, which fhone like burnifhed copper, and flafhed like lightning at every impulfe of the wind. The whole ended with a volcano, or eruption of artificial fire, in the grandeft ftile."—*See Staunton's Embaffy to China, Volume* III. *page* 73.

XV.

On the Carbonate of Potafh. By [illegible]INACHER*.

Carbonate of potafh requires to be formed by paffing the gas through a cold alkaline folution till it cryftallizes, and not by evaporation.

CHEMISTS know that carbonate [illegible] potafh well faturated, fo as to effloresce, can only be formed by making pafs through a cold alcaline folution a [illegible] of carb[illegible] acid fufficient to caufe a fpontaneous cryftallization. [illegible] on ftopping the difengagement of gas at the moment [illegible] the earth of the alkali appears to be depofited, [illegible] ating the liquor by a mild heat, there are only larg[illegible] [illegible]als obtained, which foon deliquefce.

The gentle heat of Curaudeau does not fucceed.

Alkaline ley warmed by h[illegible] tan[illegible] ccording to the method of M. Curaudeau, do[illegible] fucc[illegible]ed any better in forming a well faturated alkal[illegible] evapora[illegible]n. I have experienced that its cryftal[illegible] grow [illegible], and th[illegible] [illegible]thor himfelf acknowledges a flight deli[illegible]fcence.

Welter's apparatus is too complicated; and fo is that of Pelletier.

It is generally agreed, that th[illegible] apparatus of Welter, defcribed in the twenty-feventh Vo[illegible]me of the Annales de Chimie, is too complicated, and that of M. Pelletier has been adopted in its place in alm[illegible] all laboratories, with fome alterations in the difpofition of the firft bottle, to which is fixed a tube with a double or triple perpendicular curvature, for the introduction of the acid, or a long pipe of glafs terminated in a tunnel. The chalk mixed with water to the confiftence of thin foup, is poured by degrees into this pipe, which is ftopped by a glafs rod accordingly, and the gas is forced to traverfe the bottles containing the alkaline ley.

* From the Annales de Chimie, Tom. LV.

This

This method is in my opinion attended with much inconvenience, for when a tube with many perpendicular flexures is used, it must be charged with a column of the fluid sufficient to counterbalance the pressure of the carbonic acid gas, and consequently to give an elevation which exposes it to be easily broken; and when a long horizontal pipe of glass is employed, it often happens that the chalk is exploded into the air, when the piston is opened.

The reason stated.

Another method appears to be more simple and commodious than this, which is something like that of M. Brugnatelli, but the Italian chemist has not published the details of his method, without which it is impossible to be followed.

Simpler method not yet described.

A kilogram of chalk in powder is to be put into a bottle with two or three necks, capable of containing 12 kilograms of water; on this is to be poured a litre of a mixture of one and a half kilogram of vitriolic acid, with nine kilograms of spring water. the gas is expelled. and a crust of sulphate is formed at the surface of the calcareous carbonate. At the end of two hours all the rest of the acid water is to be added, and the bottle stopped quickly. Bubbles of the gas will be rapidly disengaged, but they will by degrees be discharged more slowly, and continue to be so moderately for twenty-four hours; then the mixture is to be stirred with an iron rod, and the gas will continue to be developed for 24 hours more, with little interruption.

I found the term of the effervescence prolonged by the resistance which was opposed to the action of the sulphuric acid on the chalk by the density which the combination necessarily acquired; which density the tendency of the sulphuric acid to augment the solubility of the sulphate of lime, fixed to proper limits.

As my apparatus, with the exception of the first bottle, is the same of that of M. Pelletier described in the fifteenth Volume of the Annales, I shall not speak of it, but only make the following remarks:

1. Pelletier was wrong in neglecting to use an intermediate bottle half filled with water: which is very necessary to separate the sulphuric acid which the gas always brings over.

2. The tubes of an inch diameter, being too difficult to be bent, may be replaced by others of seven or eight lines aperture, which will do equally well.

3. The

3. The alkaline solution, made by two lb. alkali to three lb. water, crystallizes too quick, and before the precipitation of the silex.

4. The doses of alkali and water most favourable to a regular crystallization, at the temperature of from 5 to 10 above 0 of Reaumur, are one part of distilled water, and half a part of purified potash.

If the results of my experiments shall improve the preparation of carbonate of potash, and if the disposition of my apparatus prevents the necessity of continually watching its direction, by procuring without trouble that gentle and continued pressure, of which Pelletier perceived the efficacy for the saturation of the alkali, I have reason to think that the true friends of chemistry, I mean those who practise it, will consider my observations with indulgence.

XVI.

*Method of preparing a luminous Bottle, which long preserves its Effect.**

A luminous bottle. Put a small piece of phosphorus in a long phial: pour on it boiling oil. The fluid will give light in the vacuous space whenever the cork is pulled out.

IT is easy to prepare a bottle which shall give sufficient light during the night to admit of the hour being easily seen on the dial of a watch, as well as other objects, by the following means.

A phial of clear white glass, of a long form, should be chosen, and some fine olive oil should be heated to ebullition in another vessel: A bit of phosphorus, of the size of a pea, should be thrown into the phial, and the boiling oil should then be carefully poured over it, till the phial is one-third filled: The phial should then be carefully corked; and, when it is to be used, it should be unstopped, to admit the external air, and closed again: The empty space of the phial will then appear luminous, and give as much light as a dull ordinary lamp. Each time that the light disappears, on removing the stopper it will instantly re-appear. It is proper to observe, that in cold weather it will be necessary to warm the bottle for a little while in the hands before the stopper is removed, without which precaution it would not yield any light.

* Sonini's Journal.

A phial

A phial thus prepared may be used every night for six months: there is no danger of fire from it, and its cost is very small.

XVII.

Analysis of the Magnesian Earth of Baudissero in Canavais (in the Department of the Loire,) known by the Name of Porcelain Earth, and hitherto considered as a Clay. By M. Giobert.*

THE earth of Baudissero, known by the name of porcelain earth, has been hitherto considered as one of the purest argillaceous earths known in the history of fossils, and is arranged in our cabinets of minerals as native alumine.

Porcelain earth considered as native alumine.

In a manufacture of stone-ware pottery, which has been established at Vineuf, this earth has been used for a long time, as a clay of extraordinary purity. The celebrated Macquer, and with him Baume, to whom specimens of this earth were sent from the above manufacture, pronounces positively that it was a clay of superior quality to that which they used in the manufactory of porcelain at Sevres.

Used as such in the stone-ware pottery at Vineuf.

Various chemists adopted the same conclusion.

Doctor Gioanetti continued to use it in the manufacture of his fine porcelain at the same Vineuf; and he engaged in, if not an analysis, at least some experiments on this earth, to ascertain more precisely the proportions in it between silex and earth, which he believed to be pure alumen. These experiments convinced Doctor Gioanetti, that, with the exception of a little carbonic acid which he found in it, the earth of Baudissero was an alumen almost perfectly pure, or at least the purest that he had ever met with.

This chemist, when I made enquiries of him relative to this earth, assured me frequently, that picked pieces yielded him sometimes ninety *per cent.* of alumen, including a little carbonic acid, and that in the gross it yielded constantly at least 80.

The alumine was estimated at 80 per cent.

On the perusal of the mineralogical description of the mountains of Canavais by the Chevalier Napion, it will be found that this estimable mineralogist has not hesitated to declare the

Other authority.

* Journal de Physique, LX.

earth

earth of Baudiſſero to be the moſt pure alumen ever found in Piedmont; and again in his elements of mineralogy, he mentions the earth of Baudiſſero as native alumen.

Contrary to theſe aſſertions this native earth contains no alumine at all.

Facts ſo poſitively aſſerted by ſcientific men ſo eſtimable as Maquer, Baume, and our colleagues Gioanetti and Napion, admitted no doubt of the nature of this earth; to which authorities might be added the ſucceſs with which Gioanetti conſtantly uſed it in his porcelain manufacture.

Among a number of reſearches which I made relative to the artificial fabrication of ſulphate of alumen, I employed myſelf on this earth, and to my great ſurprize found that the earth of Baudiſſero not only was not pure alumen, but did not even contain an atom of it.

Immenſe quantities of ſulphuret of iron at Baudiſſero.

The town of Baudiſſero is ſituated at leaſt three leagues from Ivree and from Brozo, this laſt village, as celebrated for its iron mines as for the manner in which they are wrought, contains in a mountain, among other minerals, an inexhauſtible maſs of ſulphuret of iron of a remarkable purity, where there is eſtabliſhed a manufacture of ſulphate of iron by the combuſtion of the ſulphur.

Efflorеſcence of the neighbouring blocks of ſtone by ſulphureous vapours.

On inſpecting this manufacture laſt year, I was ſtruck with the ſingularly powerful action, which the ſulphureous acid, formed by the combuſtion of the ſulphur, (and of which a part expanded itſelf to neighbouring places,) exerciſed on the great blocks of ſtone.

Theſe ſtones were a ſort of granite ſchiſtus; and the ſulphureous acid attacked them ſo forcibly that it made them exfoliate, and at laſt reduced them to a ſpecies of eﬄoreſcence, or white powder evidently ſaline, in which its aſtringent taſte announced ſulphate of alumen.

Probability that ſulphate of alumine or alum might be advantageouſly made from the porcelain earth.

This circumſtance made me think that if a good argil was expoſed to the action of the acid it would be alumenated; and the earth of Baudiſſero, which I believed to be almoſt pure alumen, being at ſuch an inconſiderable diſtance, made me conceive the hope of being able to eſtabliſh with economy at Piedmont, a manufacture of artificial ſulphate of alumen.

Very promiſing local advantages.

The idea of this eſtabliſhment appeared to me to be ſo much the more fortunate as there was at the foot of the ſame mountain which contained the pyrites, a great turbary, which extended almoſt as far as Chinſella, that is to ſay, almoſt to Baudiſſero, and which might furniſh fuel at a very ſmall expenſe;

pense; and it seemed to me that nature, in placing at one side an inexhaustible mine of sulphur, and at the other inexhaustible masses of the proper earth of an extremely rare purity, and between them an abundant supply of fuel, of the sort most proper for this kind of work, had done its utmost in favour of the establishment I intended.

Experiments preparatory.

There only remained to make some experiments with a view to ascertain the most profitable way of proceeding; and to examine principally if the iron which is united to the sulphur in the pyrites, would not be injurious to the sulphate of alumen obtained.

With this design I began by examining the action of the earth of Baudissero on the sulphate of iron, and the quantity of the earth necessary for the decomposition of a given weight of sulphur.

The earth of Baudissero decomposed sulphate of iron in the humid way. A little potash was added and the liquor set to crystallize.

In the different experiments the sulphate of iron dissolved in water, and boiled with this earth in different proportions became evidently decomposed after boiling for less than a quarter of an hour; the iron was precipitated of a blackish grey, while the solution was colourless, and ammonia dropped into it formed only a very white precipitate, which did not announce much iron: I filtered the liquor, of which one part was mixed with a little potash, and placed so as to crystalize; and to ascertain whether there was any potash in the earth of Baudissero, I set another part to crystalize without any alkali.

It gave sulphate of magnesia and not allum.

I observed that the liquors crystallized immediately after becoming cold; but in the place of octahedrons, I found the most perfect, the most beautiful, and purest crystals of sulphate of magnesia.

All the crystals were of this kind, and hence magnesia is an excellent porcelain earth.

The liquor which remained produced, on a new evaporation, the same pure crystals of sulphate of magnesia; and did the same on successive evaporations and crystalizations to the last drop of the liquor. In this manner was a natural alumen transformed entirely into magnesia, and at the same instant magnesia became at once an excellent porcelain earth. If examples of this kind should multiply, the necessity of chemical analysis for the knowledge of fossils will become more and more manifest, and less reliance will be learned to be placed on their external and physical characters, which at present seems to me to be too much abused.

The

More careful examination of the native earth.

The above unexpected results engaged me to make a more careful examination of the earth of Baudissero, and which is the object of this memoir.

At the time when I found that the supposed alumen of Baudissero in Canavais, was really a magnesian earth, I knew of no other example of an earth truly magnesian, but that of the earth of Salinelle, or of Sommieres, which Berard had made known (*Annales de Chimie, Tome* XXXIX. p. 65.)

Other specimens of native magnesia.

In this magnesian earth there is no mixture of any other earth except silex, and that in a very small proportion, of which fact there are but few examples. But on receiving the twelfth volume of Brochant's Mineralogy, I found that the discovery of a magnesian earth was announced in it, which is the native carbonate of magnesia found by Doctor Mitchel at Roubschitz in Moravia. From the analysis which he made of it, and which is mentioned by Brochant, we are assured that the native carbonate of magnesia of Moravia is composed only of magnesia and carbonic acid in almost equal parts; but the yellowish grey colour spotted with black, which Doctor Mitchel gives to this earth, seems sufficiently to indicate the existence of some other constituent parts. On comparing the characters and nature of the magnesian earth of Baudissero, it will be easy to perceive the differences which distinguish it from the other preceding magnesian earths.

Local situation of the earth of Baudissero.

The magnesian earth of Baudissero is found disposed in a vein in a steatite rock, of which the mountain is composed that encloses it. It is accompanied by an horn-stone, sometimes of a transparent pale colour, sometimes, when its decomposition commences, of a white almost opaque. In this state the horn-stone does not appear to be that of which Doctor Bonvoisin has given the description and analysis, under the name of the Hydrophane of Piedmont.

It is found in masses, lumps, or fragments.

Our magnesian earth appears in masses, sometimes in roundish lumps (mamelonnés) and sometimes in fragments more or less large; the lumps and fragments are sometimes, but rarely, tuberculose.

Beautifully white.

This earth is of the most beautiful white, in which it differs from that of Moravia, of which the colour is a yellowish grey spotted with black, and from that of Salinelle, or of Sommieres, which is of a chocolate colour.

The

The hardness of this earth is variable, sometimes it is soft, in which state I shall call it the *earthy* sort, and some pieces of it have a considerable hardness; as in all my experiments I tried them comparatively, I shall name this last variety the *stony* kind, to distinguish it from the preceding.

Soft or hard so much as to scratch steel.

The stony variety is scratched by steel, sometimes, on the contrary, it is hard enough to scratch steel. It can be easily reduced to powder; but with much difficulty to very fine powder, and this only takes place after long trituration in a mortar of porphyry. Its hardness neither increases nor diminishes by the action of the air; in this respect it differs from the magnesia of Moravia, which is very friable, and from that of Salinelle, which is soft in its bed, and only grows hard on exsiccation in the air.

Pulverable, tho' not finely, permanent in the air.

The fracture of this variety is conchoidal and unequal.

Fracture conchoidal.

Its surface is dull; sometimes, but very rarely, shining spots appear. It is constantly perfectly opaque, and moderately heavy; its specific gravity is variable.

Dull, opake, moderately heavy.

It is a little unctuous to the touch in the friable and earthy sort, and but very little so in the stony variety.

Slightly unctuous and adhesive.

It sensibly adheres to the tongue, though not much; it acquires this property in a considerable degree when it is moderately warmed at the fire.

Plunged in water, the stony variety does not absorb it at all; the friable sort absorbs it greedily, and with an hissing, but the mixture does not grow hot.

The soft specimens absorb water and mix like clay.

The friable species mixes with water to a considerable degree, in the same manner as clay; the fine particles of this earth, like those of clay, continue a long time suspended in water, with this difference from those of clay, that they do not unite together. Urged by the blow-pipe, on a cianite crystal, it is infusible.

Are not fusible by the blow-pipe.

Treated in a mass, on the fire in a crucible, especially in a red hot crucible, it soon decrepitates, and divides into thick scaly pieces, which leap out of the crucible; this does not happen if it is heated by degrees and moderately.

If it is reduced to a fine powder, and then traited on the fire, as soon as the bottom of the crucible begins to grow red hot, this earth boils for a short time, and seems to unite together, as if moderately moistened.

But apparently so in a crucible.

With loss of one seventh.

An hundred parts of this earth treated in this manner, until the boiling ceases, after an hour of incondescence, became reduced to 85, and 0,40. The earth calcined in this manner throws out that blueish light which has been observed from common magnesia.

Giving out carbonic acid.

If the calcination is made in a retort of earthen ware, to which a syphon is adapted, and plunged into a bottle filled with lime water, there is formed in the bottle carbonate of lime; so that the diminution of weight is partly due to the disengagement of carbonic acid.

It contains a minute quantity of sulphate of lime.

A thousand grains of this earth in fine powder were boiled in six pounds of distilled water. The liquor being filtered, and then essayed by various reagents, presented the following results.

With the solutions of the acetate, nitrate, and muriate of barytes, the mixture became troubled almost instantly, and formed a sediment of sulphate of barytes, but in a very small quantity.

The oxalate of ammonia formed oxalate of lime with it, but also in a very small portion.

These experiments repeated different times on the earth both of the stony and friable varieties, constantly gave the same results.

Lime and sulphuric acid, or sulphate of lime, is therefore, although in a small degree, a constituent part of the earth of Baudissero both in the stony and soft state.

A minute portion of muriatic acid seems to be present in the stony variety.

The nitrate of silver formed a precipitate equally with both sorts; but remarkable differences were observed between its effects in the water from the stony species, and on that from the soft variety; with the latter it formed a precipitate, which collected in a powder at the bottom of the glass; but with the water from the stony kind, besides the precipitate, filaments were produced constantly, which indicated the presence of muriatic acid. Many times the "sulphuric acid*" was first removed by the acetate of barytes, and after filtration, on being treated with the nitrate of silver, still formed a precipitate of muriate of silver.

* It is not clear what the removal of the *sulphuric acid* mentioned here had to do with making the appearance of the *muriate of silver* seem extraordinary; perhaps it is an error in the original; the translation is literal and correct. B.

The infusion of the stony sort afforded differences from the other, with ammonia also; this reagent did not ever trouble the infusion of the friable species, but always troubled, though slightly, the infusion of the stony variety.

It follows from these observations that, besides the sulphate of lime which both kinds of the earth of Baudissero contained, the stony variety held in union muriatic acid, perhaps in combination, partly with the lime, which there was not sufficient sulphuric acid to saturate, and partly united to another earth, which was not lime, since its solution permitted itself to be decomposed by ammonia; and it will appear that this earth was magnesia.

The sulphuric, nitric, and muriatic acids attack this earth, when it is well divided into an extremely fine powder. The ancient mineral acids attack this native earth.

Their action however is but little apparent, but on the least addition of heat it becomes strongly marked. Very small bubbles of gas, which rise from the bottom of the liquor, a slight white scum which forms itself at the surface, and a small hissing, shews plainly that there is a disengagement of an aeriform fluid or effervescence.

When the earth has been previously calcined in the fire, their action is very different. There is not, as may be foreseen, any effervescence; but the mixture grows hot, to that degree that a true ebullition ensues; in some minutes the mixture assumes a solid form, caused by a kind of jelly produced by it. With great force if previously calcined.

The acid which has the greatest action on it is the muriatic acid, and after this the nitric, and the sulphuric acid after both. This last however does not dissolve without much difficulty the whole of the soluble part, and that after a long continued ebullition. The muriatic acid acts more strongly.

The solution made in the closed vessels disposed so as that the gas may be received, forms with lime-water carbonate of lime, which confirms the disengagement of a little carbonic acid before indicated by the calcination of this earth in the fire.

The solutions of this earth in the acids are perfectly colourless.

The prussiate of lime or the oxalate of ammonia do not at all trouble them.

Ammonia forms with them an abundant precipitate. Ammonia precipitates the solution as does likewise the carbonate of potash.

The common unsaturated carbonate of potash forms also a precipitate with them.

When this carbonate ceases to trouble the liquor, and that, after being left to settle and being filtered, the clear liquor is submitted to ebullition, it becomes troubled again and throws down a second time an earthy precipitate.

But not if saturated with c. acid.

Finally, if instead of the unsaturated carbonate, the carbonate of potash, well saturated with acid is used, there is not the least precipitate formed.

The earth is pure magnesia.

Experiments, which I will relate, shew not only that the earth dissolved by the acids is magnesia, but that there is not mixed with it the least particle of lime, which can be discovered by the oxalate of ammonia, that there is no trace of alumen in it, which the saturated carbonate of potash precipitates, and does not again dissolve; that it does not contain the least oxide of iron, that can be indicated by the prussiate of lime; and finally, that it is magnesia perfectly pure.

As is shewn by the sulphate.

This result is farther confirmed by the sulphate of magnesia, which the crystallization of the solution of this earth in sulphuric acid yields exclusively.

Insoluble residue or silex about one 6th.

The acids in dissolving this earth leaves a residue, the quantity of which seems to be variable; that which the sulphuric acid leaves is constantly more than what is left by the muriatic or nitric acids. An hundred and twenty grains of this earth, after being well lixiviated in pure water, left a residue of which the weight, in the different experiments which I made, did not ever exceed 17 grains, and never was less than 14. The stony variety was that which in general gave the most of this insoluble residue. Many experiments, which I have made, and which it would be useless to repeat here, have convinced me that this residue is perfectly pure silex.

Component parts recapitulated.

The earth of Baudissero, from the preceding experiments, consists entirely of magnesia with a little carbonic acid, a small quantity of silex, and a very minute portion of sulphate of lime, with, in the stony variety, some traces of muriate of magnesia.

(To be concluded in the Supplement.)

Experiments

XVIII.

Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr. HARDING'S *lately discovered Star. By* WILLIAM HERSCHEL, *L. L D. F. R. S. Abridged from the Phil. Transf.* 1805.

THE discovery of Mr. Harding having added a moving celestial body to the list of those that were known before, Dr. Herschel was desirous of ascertaining its magnitude: and as in the observations which it was necessary to make he intended chiefly to use a ten-feet reflector, it appeared to be a desideratum highly worthy of investigation to determine how small a diameter of an object might be seen by this instrument. It is known that a very thin line may be perceived, and that objects may be seen when they subtend a very small angle; but the case he wanted to determine related to a visible disk; a round, well defined appearance, which might without hesitation be affirmed to be circular, if not spherical.

Enquiry as to the smallest angle under which the eye by a telescope can determine the figure of an object.

In April of the year 1774, the Doctor determined a similar question relating to the natural eye: and found that a square area could not be distinguished from an equal circular one till the diameter of the latter came to subtend an angle of 2′ 17″.

The author's unassisted eye cannot distinguish a smaller circle than of 2′ 17″ from an equal square.

He did not think it right to apply the same conclusions to a telescopic view of an object, and therefore had recourse to the a series of experiments.

The first course of experiment, was made with the heads of pins deprived of their polish by tarnishing them in the flame of a candle. The diameters of the heads were measured by a microscopic projection, with a magnifying power of 80. These measures were so exact, than when repeated they seldom differed more than a few ten thousandths part of an inch from each other. The focal length of the mirror on Arcturus is 119,64 inches, but on these objects 125,9, and the distance was measured with deal rods.

Experiments with the telescope: the objects were pin's heads.

And the result of this experiment was that an object having a diameter ,0425 could be easily seen in the author's ten-foot telescope to be a round body, when the magnified angle under which

which it appeared was 2′ 18″,9, and that with a high power (522) a part of it, ſubtending an angle of 0″,364 may be conveniently perceived.

When the purpoſe of this experiment, was conſidered, the reſult was not found ſufficient to anſwer the intention; for as the ſize of the object required the uſe of a low power, a doubt aroſe whether the inſtrument would be equally diſtinct when a higher ſhould be required. To reſolve this queſtion, it was neceſſary either to remove my objects to a greater diſtance, or to make them ſmaller.

With globules of ſealing-wax.

2. Small globules of ſealing-wax were therefore made by dipping the point of a fine needle, a little heated, into it, which took up a ſmall globule. To prevent ſeeing them at a diſtance in a different aſpect from that in which they were meaſured under the microſcope, the needles were fixed with ſealing-wax on ſmall ſlips of cards before the meaſures were taken.

By this experiment it was found, that with a globule ſo ſmall as ,00763 of a ſubſtance not reflecting much light, the magnified angle muſt be between 4 and 5 minutes before we can ſee it round. But it alſo ſhewed that a teleſcope with a ſufficient power (522) will ſhow the diſk of a faint object when the angle it ſubtends as the neked eye is no more than 0″,653.

Globules of ſilver.

The third experiment was made with globules of ſilver. As the objects made of ſealing-wax, on account of their colour, did not appear to be fairly ſelected for theſe inveſtigations, a ſet of ſilver ones were made. They were formed by running end of ſilver wires, the 305th and 340th part of an inch diameter, into the flame of a candle.

By this experiment it was found that the teleſcope acted very well with a high power (522), and will ſhow an object ſubtending only 0″,484 ſo large that it may be divided into quarters of its diameter,

Experiments with other globules, and with ſilver at greater diſtances, &c.

The fourth experiment was made with globules of pitch, bee's-wax, and brimſtone, and did not prove ſo generally advantageous as thoſe with ſilver which reflected more light.

And a fifth and ſixth experiment was made with the ſilver globules at greater diſtances; and by illuminations at night by the flame of a lamp of which for brevity the particulars are here omitted.

The

The author then proceeded to make direct obſervations on the ſpurious diameters of celeſtial bodies, from which he deduces the following reſults:*

Spurious diameters of ſtars greater than real.

(1.) As the diameters of fixed ſtars are undoubtedly ſpurious, it follows that, with the ſtars, the ſpurious diameters are larger than the real ones, which are too ſmall to be ſeen.

Sizes are the ſame in like circumſtances;

(2.) From many eſtimations of the ſpurious diameters of the ſtars † it follows, not only that they are of different ſizes, but alſo that under the ſame circumſtances, their dimenſions are of a permanent nature.

and colours.

(3.) By this and many other obſervations it appears, that the ſpurious diameters of the ſtars are differently coloured, and that theſe colours are permanent when circumſtances are the ſame.

They are leſs with high powers;

(4.) By many obſervations, a number of inſtances of which may be ſeen in Dr. Herſchel's catalogues of double ſtars, their ſpurious diameters are leſſened by increaſing the magnifying power, and increaſe when the power is lowered.

but not proportionally.

(5.) It is alſo proved by the ſame obſervations, that the increaſe and decreaſe of the ſpurious diameters, is not inverſely as the increaſe and decreaſe of the magnifying power, but in a much leſs ratio.

Small ſtars are moſt enlarged.

(6.) The magnifying power acts unequally on ſpurious diameters of different magnitudes; leſs on the large diameters, and more on the ſmall ones.

Leſs aperture cauſes greater ſp. diameter; and ſmall ſtars are moſt affected by this change.

(7.) When the aperture of the teleſcope is leſſened, it will occaſion an increaſe of the ſpurious diameters, and when increaſed will reduce them.

(8.) It alſo ſhows that the increaſe and decreaſe of the unequal ſpurious diameters, by an alteration of the aperture of the teleſcope, is not proportional to the diameters of the ſtars:

(9.) But that this alteration acts more upon ſmall ſpurious diameters, and leſs upon large ones.

Very ſmall ſtars appear nebulous.

(10.) From this we find that ſtars, when they are extremely ſmall, loſe their ſpurious diameters, and become nebulous.

* On this ſubject ſee our Journal, Vol. VI. p. 15, and Fig. 1, Plate IV.

† See Catalogue of double Stars. Phil. Tranſ. for 1782, p. 115; and for 1785, p. 40.

(11.) Many

Other causes affect the spurious diameters.

(11.) Many other causes will have an influence on the apparent diameter of the spurious disks of the stars, such as the goodness of the specula and lenses; but they are so far within the reach of our knowledge, that with a proper regard to them, the conclusion he has drawn in Rem. (2.) "that under the same circumstances their dimensions are permanent," will still remain good.

Similar experiments were made on the spurious diameters of terrestrial objects, namely silver globules, which afforded the following results:

Spurious disks of globules are smaller than the real disks.

(1.) The terrestrial spurious disks of globules are less than the real disks; whereas we have seen, in Remark (1.) of the celestial spurious disks, that these are larger than the real ones.*

Larger magnitudes give larger sp. disks;

(2.) The less globule has the largest spurious disk. This agrees with the spurious disks of celestial objects: the stars of the first, second, and third magnitude, having a larger spurious disk than those that are of inferior magnitudes.

coloured like the celestial:

(3.) With respect to colours, the terrestrial also agree with the celestial spurious disks.

Less with greater mag. power;

(4.) The spurious diameters of the globules, like the spurious disks of the stars, are proportionally lessened by increasing the magnifying power, and increased when the power is lowered.

But not proportionally.

(5.) When the estimations are compared with the powers, it will also be seen that the increase and decrease of the spurious disks of the globules is not inversely as the powers, but in a much less ratio.

Power acts more on small than large sp. disks,

(6.) The effect of magnifying power is unequally exerted on spurious diameters; and that, as with celestial objects, so with terrestrial, this power acts more on the small spurious disks than on the large ones.

and diminution of aperture;

(7.) The spurious terrestrial disks also resemble those of the stars, by increasing when the aperture is lessened, and decreasing when it is enlarged.

greater on small disks.

(8.) By these experiments it is proved, that the increase and decrease of the diameters occasioned by different apertures is not proportional to the diameters of the spurious disks.

* It appears from the context, that this arises from the terrestrial spurious disks being formed by the small spot of reflected light from the metallic globule, and not from its whole diameter.

(9.) But that the change of the apertures acts more on the small, and less on the large ones.

(10.) The spurious disks of globules are lost for want of proper illumination, but do not change their magnitude on that account. The brightness of the atmosphere in a fine day is sufficient to produce them; though the illumination of the sun is generally the principal cause of them.

These disks are not changed by diminution of light.

(11.) The diameters of spurious disks are liable to change from various causes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally polished in its surface, or of equal convexity every where, being very seldom perfectly spherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the spurious disks will generally return to its former size.

Globules of mercury were used instead of those of silver, and with the same results.

Mercurial globules.

The spurious terrestrial disks were then measured by comparing them with circles on a tablet: and it was found that a variation in their illumination did not affect their magnitude. It was also found that the rays from the central part of the mirror gave a larger image than those from its circumference. So that when a central aperture of three inches gave an image corresponding with a circle of 0.465 inch, an annular opening from 6.5 to 8.8 inches gave only 0.18 inch for the image: and the experiments were sufficiently varied as to the magnitudes and situations of the apertures to shew that this difference did not arise from more or less light.

Measurement of spurious disks. They may be distinguished from real disks by using first a central, and then an annular aperture. The first enlarges and the second diminishes them.

This property of the mirror serves admirably to distinguish a spurious disk from a real one; and proved to be so on trial with terrestrial and celestial objects.

Trials.

Observations on the Nature and Magnitude of Mr. HARDING'S *lately discovered Star.*

On the day Dr. Herschel received an account of Mr. Harding's new star, which was the 24th of September, he directed his telescope to the calculated place of the new object, and noted all the small stars within a limited compass about it. They were then examined with a distinct high magnifying power; and since no difference in their appearance was

Observations on the planet Juno.

Observations on the planet Juno.

was perceivable, it became necessary to attend to the changes that might happen in the situation of any one of them. They were delineated as in Fig. 1, (Plate XIV.) which is a mere eye-draught, to serve as an elucidation to a description given with it in the journal; and the star marked *k*, was the new object.

Sept. 29. Being the first clear night, he began a regular series of observations: and as the power of determining small angles, and distinctness in showing minute disks, whether spurious or real, of the instrument he used on this occasion, had been sufficiently investigated by the foregoing experiments, there could be no difficulty in the observation, with resources that were then so well understood, and have now been so fully ascertained.

"Mr. Harding's new celestial body precedes the very small star in Fig. 3, between 29 and 33 Piscium, and is a little larger than that star; it is marked A. *f g h* are taken from Fig. 1. I suppose *g* to be of about the 9th magnitude, so that the new star may be called a small one of the 8th."

With his ten-feet reflector, power 496.3, he viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger star.

By way of putting this to a trial, he changed the power to 879,4, but could not find that it magnified the new one more than it did the stars *g* and *h*.

"I cannot perceive any disk; its apparent magnitude with this power is greater than that of the star *g*, and also a very little greater than that of *h*; but in the finder, and the night-glass *g* is considerably smaller than the new star, and *h* is also a very little smaller."

He compared it now with a star which in the finder appeared to be a very little larger; and in the telescope with 879,4 the apparent magnitude of this star was also larger than that of the new one.

"As far as I can judge without seeing the asteroids of Mr. Piazzi and Dr. Olbers at the same time with Mr. Harding's, the last must be at least as small as the smallest of the former, which is that of Dr. Olbers."

"The star *k*, Fig. 1, observed Sept. 24, is wanting, and was therefore the object I was in search of, which by computation must have been that day in the place where I saw it."

"The new ſtar being now in the meridian with all thoſe to which I am comparing it, and the air at this altitude being very clear, I ſtill find appearances as before deſcribed: the new object cannot be diſtinguiſhed from the ſtars by magnifying power, ſo that this celeſtial body is a true aſteroid." Obſervations on the planet Juno.

Mr. Bode's ſtars 19, 25 and 27 Ceti are marked 7m, and by comparing the aſteroid, called Juno, with theſe ſtars, it has the appearance of a ſmall one of the 8th magnitude.

With regard to the diameter of Juno, the author remarks that had it been half a ſecond, he muſt have inſtantly perceived a viſible diſk. Such a diameter, when he ſaw it magnified 879,4 times, would have appeared under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have eſcaped his notice.

Oct. 1. Between flying clouds, the aſteroid was ſeen, which in its true ſtarry form has left the place where it was ſeen Sept. 29. It has taken the path in which by calculation it was expected to move. This aſcertains that no miſtake in the ſtar was made when laſt obſerved.

Oct 2, 7h. Mr. Harding's aſteroid is again removed, but is too low for high powers.

8h 30′. Viewed it now with 220,3 288,4 410,5 496,3 and 879,4. No other diſk was viſible than that ſpurious one which ſuch ſmall ſtars have, and which is not proportionally magnified by power,

With 288,4, the aſteroid had a larger ſpurious diſk than a ſtar which was a little leſs bright, and a ſmaller ſpurious diſk than another ſtar that was a little more bright.

Oct 5, with 410,5. The ſituation of the aſteroid is now as in Fig. 4. Its diſk, which is probably the ſpurious appearance of ſtars of that magnitude, was compared with a larger, an equal, and a ſmaller ſtar. It was leſs than the ſpurious diſk of the larger, equal to that of the equal, and larger than that of the ſmaller ſtar. The gradual difference between the three ſtars is exceedingly ſmall.

"With 496,3, and the air uncommonly pure and calm, I ſee ſo well that I am certain the diſk, if it be not a ſpurious one, is leſs than one of the ſmalleſt globules I ſaw this morning in the tree."

 The

Observations on the planet Juno.

The diameter of this globule was ,02. It subtended an angle of 0″,429, and was of sealing-wax; had it been a silver one, it would have been still more visible.

With 879,4. All comparative magnitudes of the asteroid and stars, remain as with 496,3.

The minute double star *q* Ophiuchi * was seen in high perfection, which proves that the air is clear, and the telescope in good order.

The asteroid being now in the meridian, and the air very pure, the comparative diameter seems a little larger than that of an equal star, and its light also differs from star-light. Its apparent magnitude, however, can hardly be equal to that of the smallest globule observed this morning. This globule measured ,01358, and at the distance of 9620,4 inches subtended an angle of 0″,214.

When the asteroid was viewed with 879,4, more haziness was found than an equal star would have given: but this the Doctor ascribes to want of light. What he calls an equal star, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. Harding's asteroid has continued its retrograde motion. The weather is not clear enough to allow the use of high powers.

Oct. 8. If the appearance resembling the spurious disks of small stars, which I see with 410,5 in Mr. Harding's asteroid, should be a real diameter, its quantity then by estimation may amount to about 0″,3. This judgment is founded on the facility with which I can see two globules often viewed for this purpose.

The angle of the first is 0″,429, and of the other 0″,214; and the asteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill-defined hazy appearance, but nothing that may be called a disk visible. When there is a glimpse of more condensed light to be seen in the centre, it is so small that it must be less than two-tenths of a second.

To decide whether this apparent condensed light was a real or spurious disk, he applied different limitations to the aperture of the telescope, but found that the light of the new star was too feeble to permit the use of them. From this he concluded

* See Cat. of double Stars, I. 87.

that

that an increase of light might now be of great use, and viewed the asteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of these large mirrors is very seldom the same as that of the air in which they are to act, and till a perfect uniformity takes place no high powers can be used. Observations on the planet Juno.

The asteroid in the meridian, and the night beautiful. After many repeated comparisons of equal stars with the asteroid, I think it shows more of a disk than they do, but it is so small that it cannot amount to so much as 3-tenths of a second, or at least to no more.

It is accompanied with rather more nebulosity than stars of the same size.

The night is so clear, that I cannot suppose vision at this altitude to be less perfect on the stars, than it is on day objects at the distance of 800 feet in a direction almost horizontal.

Oct. 11. By comparing the asteroid alternately and often with equal stars, its disk, if it be a real one, cannot exceed 2, or at most 3-tenths of a second. This estimation is founded on the comparative readiness with which every fine day I have seen globules subtending such angles in the same telescope, and with the same magnifying power.

"The asteroid is in the meridian, and in high perfection. I perceive a well defined disk that may amount to 2 or 3-tenths of a second; but an equal star shows exactly the same appearance, and has a disk as well defined and as large as that of the asteroid."

Result and Application of the Experiments and Observations.

We may now proceed to draw a few very useful conclusions from the experiments that have been given, and apply them to the observations of the star discovered by Mr. Harding; and also to the similar stars of Mr. Piazzi and Dr. Olbers. These kind of corollaries may be expressed as follows.

(1.) A 10-feet reflector will shew the spurious or real disks of celestial and terrestrial objects, when their diameter is ¼ of a second of a degree; and when every circumstance is favourable, such a diameter may be perceived so distinctly, that it can be divided by estimation into two or three parts.

(2.) A

Observations on the planet Juno.

(2.) A disk of ¼ of a second in diameter, whether spurious or real, in order to be seen as a round, well defined body, requires a distinct magnifying power of 5 or 6 hundred, and must be sufficiently bright to bear that power.

(3.) A real disk of half a second in diameter will become so much larger by the application of a magnifying power of 5 or 6 hundred, that it will be easily distinguished from an equal spurious one, the latter not being affected by power in the same proportion as the former.

(4.) The different effects of the inside and outside rays of a mirror, with regard to the appearance of a disk, are a criterion that will show whether it is real or spurious, provided its diameter is more than ¼ of a second.

(5.) When disks, either spurious or real, are less than ¼ of a second in diameter, they cannot be distinguished from each other; because the magnifying power will not be sufficient to make them appear round and well defined.

(6.) The same kinds of experiments are applicable to telescopes of different sorts and sizes, but will give a different result for the quantity which has been stated at ¼ of a second of a degree. This will be more when the instrument is less perfect, and less when it is more so. It will also differ even with the same instrument, according to the clearness of the air, the condition, and adjustment of the mirrors, and the practical habits of the observer.

XIX.

Account of some new Improvements on Steam-Engines. By Mr. Arthur Woolf.

Mr. Woolf's improvements in steam-engines.

IN our eighth volume, p. 262, we gave a short account of a former improvement made by Mr. Woolf on the steam-engine, founded on a discovery that steam, of any higher temperature than that of boiling water, if allowed to pass into another vessel kept at the same temperature as the steam itself, will expand to as many times its volume, and still be equal to the pressure of the common atmosphere, as the number of pounds which such steam, before being allowed to expand, could maintain on each square inch of a safety-valve

exposed

exposed to the atmosphere: for example, that masses or quantities of steam of the expansive force of 20, 30, or 50 pounds the square inch of a common safety-valve, will expand to 20, 30, or 50 times its volume, and still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt.

Mr. Woolf's improvements in steam-engines.

In consequence of this discovery Mr. Woolf was enabled to use his steam twice (if he chose), and with complete effect; nothing more being necessary than to admit high steam, suppose of 40 pounds the square inch, into one cylinder, to work there by its expansive force, and then to allow the same steam to pass into, and expand itself in, another cylinder of forty times the size of the first, there to work by condensation in the common way. Or with only one cylinder, by admitting a proportionally small quantity of high steam into it from the boiler, Mr. Woolf, found that he could effect a considerable saving in fuel.

In this first improvement of Mr. Woolf, though the saving might be carried a considerable length, it was still necessarily limited by the strength of materials; for in the employment of high steam there must always be some danger of an explosion. Mr. Woolf, however, by a happy thought, has completely obviated every danger of this kind, and can now take the full advantage of the expansive principle without the least danger whatever. This he effects by throwing into common steam the additional temperature necessary for its high expansion, *after the steam is admitted into the working cylinder*, which is heated by means adequate to the end intended to be gained; and the advantage which he thus gains he effectually secures by a most ingenious improvement in the piston. It may be easily conceived that steam of such high rarity as Mr. Woolf employs, could not be made fully effective with the piston in common use; for in proportion to its rarity so must be the facility with which a portion of it would escape, and pass by the side of the piston to the vacuous part of the cylinder: but Mr. Woolf's contrivance seems perfectly adapted to prevent the loss of even the smallest portion of the steam.

Besides

Besides these improvements on the common steam-engine, he has also found means to apply the same principles to the old engine, known by the name of Savary's, in such a way as to render the same a powerful and œconomical engine for a great variety of purposes.

Such is the outline of Mr. Woolf's improvements on this useful engine: but, for the general information of practical engineers, we shall subjoin a more technical description, in Mr. Woolf's own words, extracted from his specification of his patent.

(To be continued.)

SCIENTIFIC NEWS.

Geometry.

Two theorems from the Horologium of Huygens.

HUYGENS has given the two following theorems in his *Horologium Oscillatorium*, which are applicable to all solid bodies: "*The center of oscillation, and that of suspension are always reciprocal to one another. The same body is always isochronal to itself, while it oscillates round parallel axis's taken at equal distances from the center of gravity.* M. Biot has given a remarkable extension to these two theorems.

extended farther by M. Biot.

All these parallel axis's form the surface of a right angled cylinder of which the axis passes through the center of gravity. But the analytical expression under which M. Biot presents the theorem of Huygens, made him instantly perceive, that an arbitrary inclination might be given to this axis, the radius of the cylinder being suitably changed at the same time; and that thus according to the different degrees of inclination of the axis, an infinity of cylinders might be obtained. The superficies of which cylinders should have the same property as that of the primitive cylinder. Besides this, the axis without changing its inclination may describe a conical surface about its primitive position, which will multiply the number of cylinders already found, as often times as right lines can be conceived to be drawn on the upper surface of the cone.

Astronomy.

M. Pictet has made an observation of an occultation of the pleiades by the moon, on the 19th of *November*, 1804, from the Observatory of Geneva. M. Pictet on the occultation of the pleiades by the moon.

An account of an occultation of π scorpion, observed on the 17th of *July*, 1803, from the summit of Casuleta, a mountain in the kingdom of Spain, was found among the papers of the late M. Mechain, which will appear in the 6th Volume of the Memoirs of the French National Institute: this is the last observation of this kind made by a man of science, whose premature loss the Institute will long regret. M. Mechain o the occultation of π scorpion,

A long succession of observations was also found among his papers, relative to the comet which he had discovered from Barcelona in 1793, which will also appear in the same publication. and of the comet in 1793.

Geography.

M. Humboldt has read before the *Institute Nationale, A Memoir on the Longitude of Mexico*, the capital of the kingdom so called. The longitude of Mexico determined accurately by M. Humboldt.

Geographers disagree with regard to the position of this important point. The considerable difference which M. Humboldt found between his first observation, and the last which had been formerly made by others before him, engaged him to repeat it as often as he could, and by different methods. The distances of the moon from the stars, and several eclipses of Jupiter's moons, always gave the same result, which is doubtlessly preferable to all those which have appeared hitherto.

Electricity.

Since the discovery of electrical conductors by Dr. Franklin, many philosophers have repeated experiments to establish the identity of electrical fire and lightning, by experiments with such insolated conductors. Conductor contrived to prevent accidents,

These experiments succeeded to the wish of all who tried them; but it was soon perceived, that they were attended with much danger: and since the death of Professor Richman, of Petersburg, who was struck by lightning from his conductor in 1753, few have ventured to repeat the experiment.

M. Beyer

can be insolated or not, at pleasure.

M. Beyer of Paris has formed in his garden an apparatus of this kind, which is very simple, and at the same time perfectly effectual without any danger: It is a conductor which can alternately at pleasure be insolated, or not insolated, and made to act either with a ball, or with a point. The communications between it and the earth are well established, and as the observations can be made at more than an hundred feet from the apparatus, there is not the least danger of any accident.

Aerostation.

Balloon projected by Mr. Robertson, 132 feet diameter, to carry up 50 men,

The celebrated Aeronaut Robertson, who ascended from Petersburg last year, is endeavouring to obtain the necessary assistance at that place for the construction of an air balloon on a very large scale; he proposes that it shall be 732 feet in diameter, which he calculates will carry up 37 ton, and which he supposes, therefore, will easily support 50 people, and all necessary accommodations for them.

and a vessel with sails, &c.

It is to have attached to it a vessel furnished with masts, sails, and every other article requisite for navigating the sea in case of accidents, and provided with a cabin for the aeronauts, properly fitted up, galley for cooking, proper stores for stowing provisions, and several other conveniencies. To render the ascent more safe, it is to take up another smaller balloon within it, and a parachute, which will render the descent perfectly gentle, if the outer balloon bursts.

and an internal balloon and parachute.

From its construction it will be calculated to remain in the air several weeks, in which time many experiments in natural philosophy, and astronomical observations may be made: It is also supposed, that geography may be considerably improved by its means, as the aeronauts will be neither stopped in their course by mountains or forests; and some have even thought, that with the assistance of the trade winds, a voyage round the earth might be made in it between the tropics. Its cost, it is calculated, will be nearly equal to that of a ship of the line.

University of Charkow.

The court of Petersburg published the act of confirmation of the University of Charkow, on the 16th of May, of which the following are the chief particulars.

The

The University is under the care of the Minister of public instruction: It has, however, its own particular administration and jurisdiction; the ordinances, by which its members are governed, are regulated by itself: It has the right of censure both with regard to the books printed by itself, or those brought from abroad. All articles which it may want are allowed to pass the frontiers without examination or tax. Its correspondence is post free, and its paper is not subject to duty. The houses of the professors are free from taxes and all charges. The professors have the rank of the seventh class, and the students that of the twelfth, or that of subaltern officers, receive commission as such, and wear swords. The professors after twenty-five years duty, or in case of incurable sickness, receive their pensions for life, and may even receive them while resident in other countries. On the death of a professor, his widow and children continue to receive his pension, until the widow marries again, or the children attain the age of twenty-one years. The Emperor has granted a yearly revenue to the University of 130,000 rubles.

Forms its own ordinances and regulations.

Various privileges.

Provision for professors, &c.

Botanic Garden, &c. at Copenhagen.

A sum of 4,500 rix dollars, which the government had granted in 1803, for the Botanic Garden at Copenhagen, has been employed, partly in paying the debts of the establishment, and partly in constructing a new hot-house. This garden, which possesses 5,500 plants of different kinds, is open one day in each week for the curious, and every day for botanic students. The directors in their last report, having made some proposals for the improvement and establishment of the garden, the government has granted them an additional sum of 4000 rix dollars, and an annual sum of 200 for repairs; and have besides settled, that the appointments of persons employed in the garden, shall be increased to 720 rix dollars, to commence this year.

has 5500 plants for inspection,

additional grants made to it.

M. Giesecke, a Prussian mineralogist, who has been a considerable time at Copenhagen, is about to be employed by government on a voyage to Greenland, where he is to pass some years in examining that country, with regard to its mineralogy and geology. Hitherto the Moravian religious missionaries have alone been able to resolve to live some years

Proposed mineralogical expedition to Greenland.

years in that country for the converſion of the natives: It will be no little honour to the ſciences, if M. Gieſecke ſhall bring himſelf to make a like ſacrifice for their advancement.

Charts printed by moveable types.

The Royal Academy of the Fine Arts, and the Mechanical Arts of Berlin, have received among their members M. F. H. Wagener, who has diſcovered a new method of printing geographical charts by a ſpecies of moveable types, which is found to anſwer better than engraving, and will undoubtedly be much cheaper.

Prizes given by the French war miniſter for

Marſhal Berthier, war miniſter of France, at the requeſt of general Mareſcot, has again eſtabliſhed the prizes which was given for the beſt works on fortification.

a work on ſubterranean fortification,

Two prizes have been granted to the beſt *treatiſes on ſubterraneous works*. The firſt was adjudged by the committee of fortification to Major Mouze, the ſecond to Captain Gillot; the committee has adjudged a third treatiſe deſerving of honourable mention, which has for its inſcription *artem experientia fecit*; the author of it is not known.

and one on a plan for a fortified barrack.

Another prize on the ſubject of a *project for a fortified barrack*, has been given to Captain Laurent. The committee have judged the project of Captain Bioſchevalier, and that of Lieutenant Colonel Geſbert, to deſerve honourable mention. The committee has rejected, for not correſponding with the propoſed ſubject, a project of Captain Mallet, for *a barrack intrenchment*; but have thought it worthy of particular mention, as a work which gives a very advantageous idea of the talents of this officer.

Many of the works which neither received prizes, or particular mention, exhibit ingenious contrivances, and intereſting obſervations. In general theſe two conteſts have fully proved the goodneſs of the Inſtitution, of which the object is to exite emulation in all the corps of the army, to propagate knowledge among them, and to extend the perfection of all the branches of the military art.

Catalogue of Leipſic fair, contained 3647 publications.

THE catalogue of the Leipſic fair, has this year contained two ſheets more than uſual. The muſical publications have been

been added to it. It contains 3,647 articles, furnished by 380 booksellers. The number of romances is 271, of theatrical pieces 81, and music 95, forms 447 articles.

The academy of painting and sculpture at Madrid, are about to publish a compleat collection of the Arabic antiquities of the kingdoms of Grenada and Cordova. In this work will be found not only views and plans of the monuments, and other remarkable matters of these countries, but also an explanation of all the inscriptions, cyphers and hieroglyphics.

Publication of Arabic antiquities of Spain.

THERE is soon to be published at Lisbon, a Dictionary of the Angola or Bunda language, with the explanation of all the words in Portuguese. There has never before been a dictionary of this language. This will be published for the use of those Portuguese who have business to transact with the establishments which their country possesses on the coast of Africa. No language is spoken there to so great an extent as this.

Dictionary of the Bunda language, serviceable to those who have business in Africa.

THE celebrated sculptor M. Canova, is engaged in erecting at Vienna, the splendid Mausoleum of the Arch-dutchess Christina, an immense composition of eight marble figures, larger than life; the models and execution of which have been long admired at Rome, where they were formed. M. Canova before his departure from Rome exhibited a colossal group, representing Theseus combating with a Centaur. This group is to be executed in marble for the city of Milan. The artists and connoisseurs of Rome seem to esteem this work superior to every other which has been executed by this ingenious and indefatigable artist.

Mausoleum of Christina, by M. Canova,

and his Theseus and Centaur.

THE Magistracy of Augsburg have had the honour of being the first government of south Germany, which have taken decisive measures against the shameful traffic of book-piracy. It has confiscated the entire edition, consisting of 500 impressions, of the work of Goener, on the political rights

Piracy of books punished at Augsburg.

rights of Germany, which was pirated by Krauzfelder, a dealer in such transactions, and has besides compelled Krauzfelder to pay to the legitimate editor, the price of the copies which he had sold.

Russian marine Institution.

THE Russian government have formed at Petersburg, an Institution, whose design is the perfecting of all that belongs to naval armaments, and which is to be called the *Marine Museum*. This institution is not merely to be a school: all the sciences necessary to a naval officer will be there taught and the *Museum* will besides publish a journal which will treat of every thing relative to the marine. It will have also a cabinet of natural history, which will be open to all the pupils. This establishment will be under the direction of the minister of the marine, and its members are to wear the naval uniform.

Russian establishments for education. Numbers of masters and pupils.

ACCORDING to the report of the minister of public instruction, there is at present in Russia 494 institutions for education, directed by 1475 masters, and attended by 33484 scholars. The expence of these establishments costs government annually almost two millions of Rubles. Among these are not reckoned those for the corps of cadets, or for pages, the Academy of Arts, the Schools of Commerce, nor the Institution for Female Education. Those who know the state in which Russian education was at the accession of Alexander, may judge by this detail what he has done towards enlightning his vast empire.

The Russian catholics earnestly concur in seconding his views. At an ecclesiastical assembly, convoked by the bishop of Lusk and Shetomir, various measures have been taken in favour of the establishments for education.

Theatre in the Crimea.

IN the town of Odessa in the Crimea, a theatre is building with much activity, according to the plans of M. Thomas de Thomon architect to the Emperor, and a professor of the Petersburg academy.

This

(This shews that the arts are even extending to this hitherto neglected part of the world, which certainly from its fine climate, and many other advantages, merits every attention of its enlightened and humane master.)

THE third volume of the *Geographical Dictionary of the Russian Empire* has been published by the booksellers, Gavy, Popow, and Luby. Geographical Dictionary of Russia.

AN important work is soon expected to appear at Petersburg, by the scientific M. Delaunay, counsellor of state, relative to Siberia, and the bordering countries.

M. KOTZEBUE in his last tour to Naples relates some particulars which he saw in visiting the Museum of Portici, which will be interesting to the admirers of ancient literature. Ancient manuscripts discovered at Portici are

" Eleven young men are at present employed in unrolling the manuscrips, and two copy them. An Englishman called Haiter, is at the head of the establishment. He relates that his assistants are much more expert and expeditious than they were formerly. He has great hope that he shall have the 600 manuscripts, (which yet remain) decyphered, and has little doubt that he shall discover among them an *Ennius* and a *Menander*, as he flatters himself he has already a *Polybius* in hands.

On the day of the visit, a Greek author, hitherto unknown named Kolotos, was discovered; his work is on philosophy. As the names of the author are always inserted at the end of each manuscript, they can never be known until it is entirely unrolled. Seven latin authors have passed through the hands of Mr. Haiter, but all so much damaged that it was impossible to unroll them, which he the more laments as one of them appeared to be a *Livy*, at least it was an historical work written in his style; all that he can discover of it, is, that it begins with an harangue, in which mention is made of the family of Acilius. They have to this time discovered five authors; *Philodemus*, most of whose writings have been found, and among others, a treatise on the *vices which are nearly allied to virtues*. *Epicurus*, *Phædrus*, *Demetrius Phalereus* the works of Epicurus, Phædrus,

and

Demetrius, and his Kolotos. and his *Kolotos* above-mentioned. Mr. Haiter regrets that he has hitherto only met with works on philofophy, although among thefe many hiftorical ideas, hitherto unknown, occur here and there: as happened in a differtation on anger, in which is cited the example of Cadmus punifhed by Bacchus for having given himfelf up to this paffion, a circumftance hitherto unknown.

IT has for fome time been an object of deliberation with me, to afcertain by what means I might moft effectually remedy an inconvenience which has arifen from the diftinguifhed patronage this Journal has been honoured with: The great extent and value of original communications cannot but be duly eftimated by the public; though at the fame time it has neceffarily followed, that various articles of news and other fubjects in the foreign Journals, have in many inftances been poftponed, and in fome rejected. To retain all the peculiar advantages of this work, and to afford ample fpace for occafional and foreign articles of value, the obvious means have appeared, that according to the practice of feveral other refpectable works, each volume fhould be concluded by a Supplementary Number; containing fix fheets, or 96 pages of printed matter, and two plates. And, as many of the former plates, like thofe in the prefent number, have contained mathematical figures or outline delineations, capable of being advantageoufly condenfed, it is purpofed in all the future numbers to give two very full plates, and fixteen extra pages of matter, inftead of the four plates hitherto given. By this arrangement every volume will in future contain 30 fheets or 480 pages of matter, and 10 full plates; inftead of 20 fheets or 320 pages, with 16 plates lefs fully occupied. This addition of new matter to the amount of full one half more, will admit the infertion of many interefting articles which want of room muft alfo have excluded.

*** *The plate of Rye-Harbour could not be finifhed in time on account of the fudden illnefs of the Engraver. It will be given gratis in the Supplement, which will be publifhed Jan.* 1. *next, at the fame time as No.* 50.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XII.

ARTICLE I.

A Description of an Air Pump upon a new Construction. By ELIZUR WRIGHT. *Communicated by* BENJ. SILLIMAN, *Esq. Professor of Natural Philosophy, &c. in Yale College, Newhaven, America.*

UPON reading the improvements made in the air pump by Smeaton, Haas, Prince, Russel, and Cuthbertson, it occurred to me that the end which they aimed at might in some measure be attained upon a principle that is different from either of those by which their pumps have been constructed It is well known that in a common air pump the valve at the bottom of the barrel depends upon the air in the receiver to open it. When the air in the receiver is rarified to a certain degree, its spring becomes too weak to overcome even the small resistance which will arise from the weight of the valve, its cohesion to the plate occasioned by the oil, and its being stretched tight over the hole. Here the progress of exhaustion will stop. And this would hold true, could it be possible to produce a perfect vacuum in the barrel. But as the same obstructions belong to the piston valve, together with the additional one arising from the pressure of the external air upon it, and because the piston can- **The general imperfections of the air pump explained.**

 not

not be so accurately fitted to the lower valve as, when put down upon it, to leave no vacuity between them; a portion of air will necessarily be retained in the barrel, which by its pressure still further prevents the opening of the lower valve, and causes the operator to come to the limit of rarefaction much sooner than he would upon the supposition that a perfect vacuum were made in the barrel. Several very ingenious contrivances have been invented to remove this imperfection, among which those of Cuthbertson and Prince are among the latest, and cannot fail of giving the reader a very high idea of their sagacity and talents for invention. The method used by the Rev. Mr. Prince, of removing the lower valve by opening the bottom of the barrel into a cistern which has a communication with the receiver, first gave the hint that it might be possible in some similar way to dispense with both the valves, and by this means carry the air pump to a greater degree of perfection. In pursuing this subject I found that all this might be effected, and in a way that admitted of much simplicity of construction.

Improvements of Cuthbertson and Prince.

The air-pump of Prince improved.

Description of a new air pump.

The principle upon which this pump operates may be seen in the following description of it. F, (*Plate* XIV. *Fig.* 1.) is the pump plate. O C is the barrel lying in a horizontal position underneath the pump plate, and nearly in contact with it. A and B are two ducts leading from the pump plate into the barrel. The piston P is without a valve, being solid and accurately fitted to the barrel. The piston rod M is cylindrical and moves air tight in the leathern collar O. There is another piston, N, made like the former, but shorter, and acted upon by the spring S, which is thence termed the spring piston. The ends of these pistons are very carefully fitted to each other, so that when they are brought into contact they will form one uniform cylinder without any vacuity betwixt them. H is the winch, with a pinion and rack by which it is worked. The pump is supported by a pedestal upon which it is firmly fixed.

A solid piston works in a barrel. Its rod passes through a collar of leather. Two holes in the barrel serve, one to admit air from the receiver and the other to discharge it.

The manner in which it operates is this: Suppose the receiver placed over the duct A, leaving the duct B open to the external air, also the spring piston in the situation N, excluding the external air from the barrel, as represented in the figure and the piston P in contact with it. The piston P, by moving towards the duct A, forms a vacuum in the barrel. When it passes by the duct A, it opens a communication between the receiver

Dr. Herschell's investigation of the power of telescopes to distinguish the figures of very minute objects.

Fig. 1.

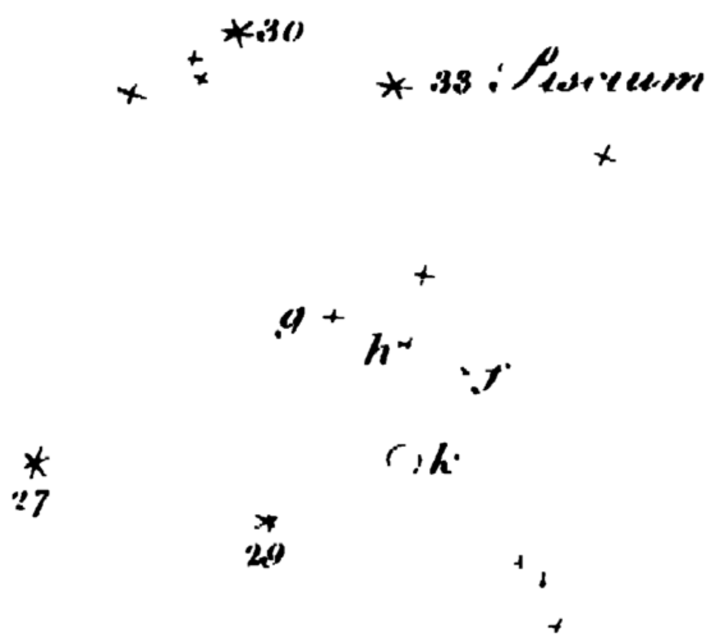


Fig. 2.

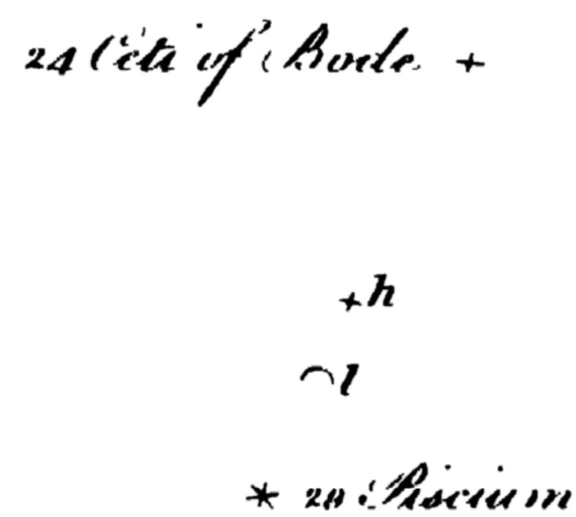


Fig. 4.

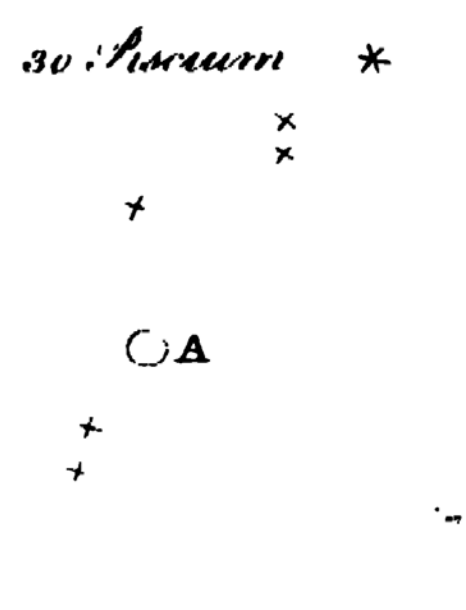


Fig. 3.

Mr. Gough's Propositions respecting a Division of the Arch of a Circle.

Fig 2.

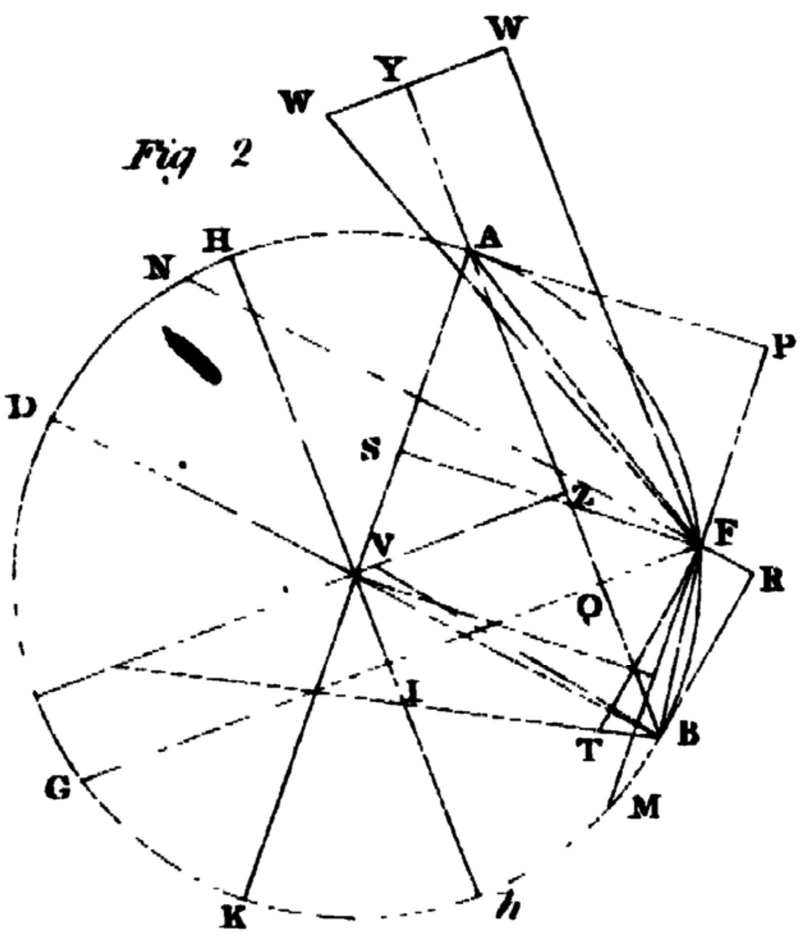


Fig 3.

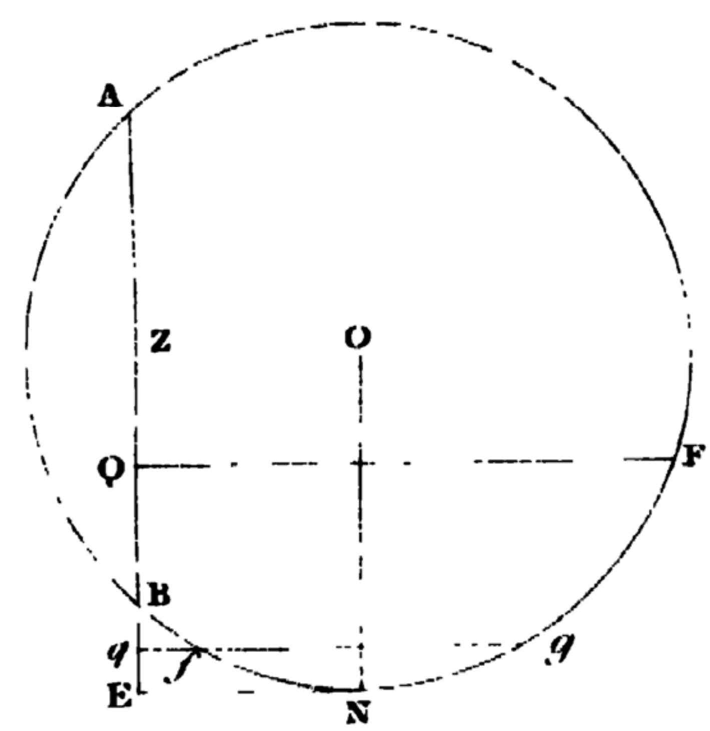


Fig 1

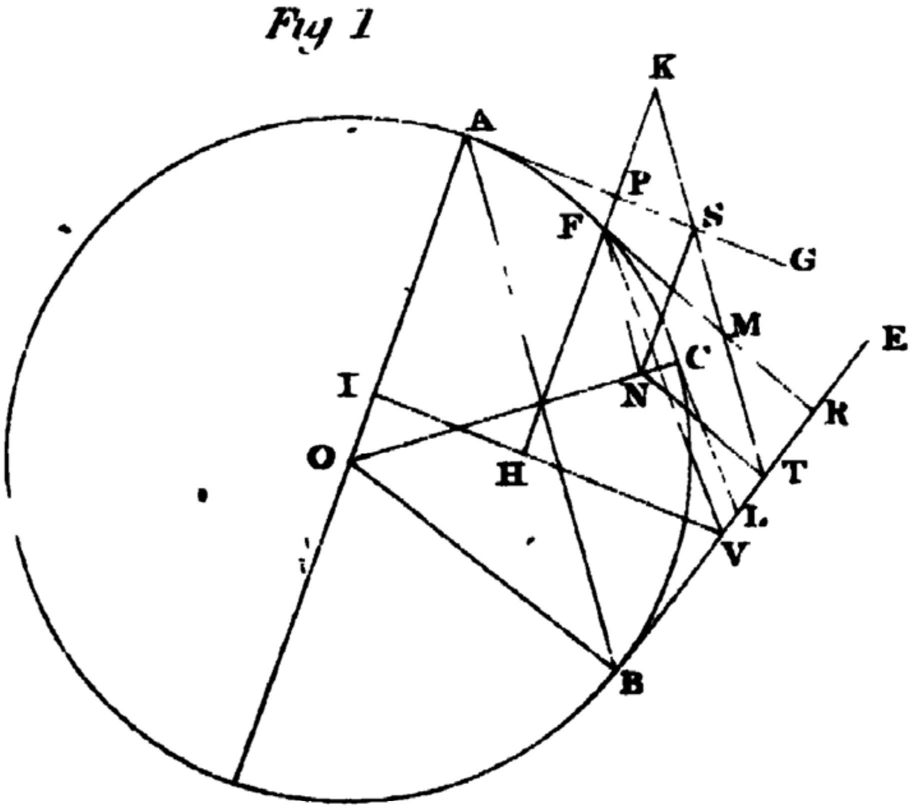
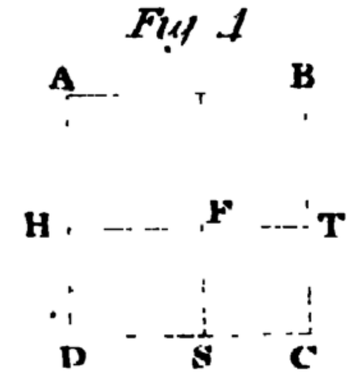


Fig 4



Mutlow Sc. Russell Ct.

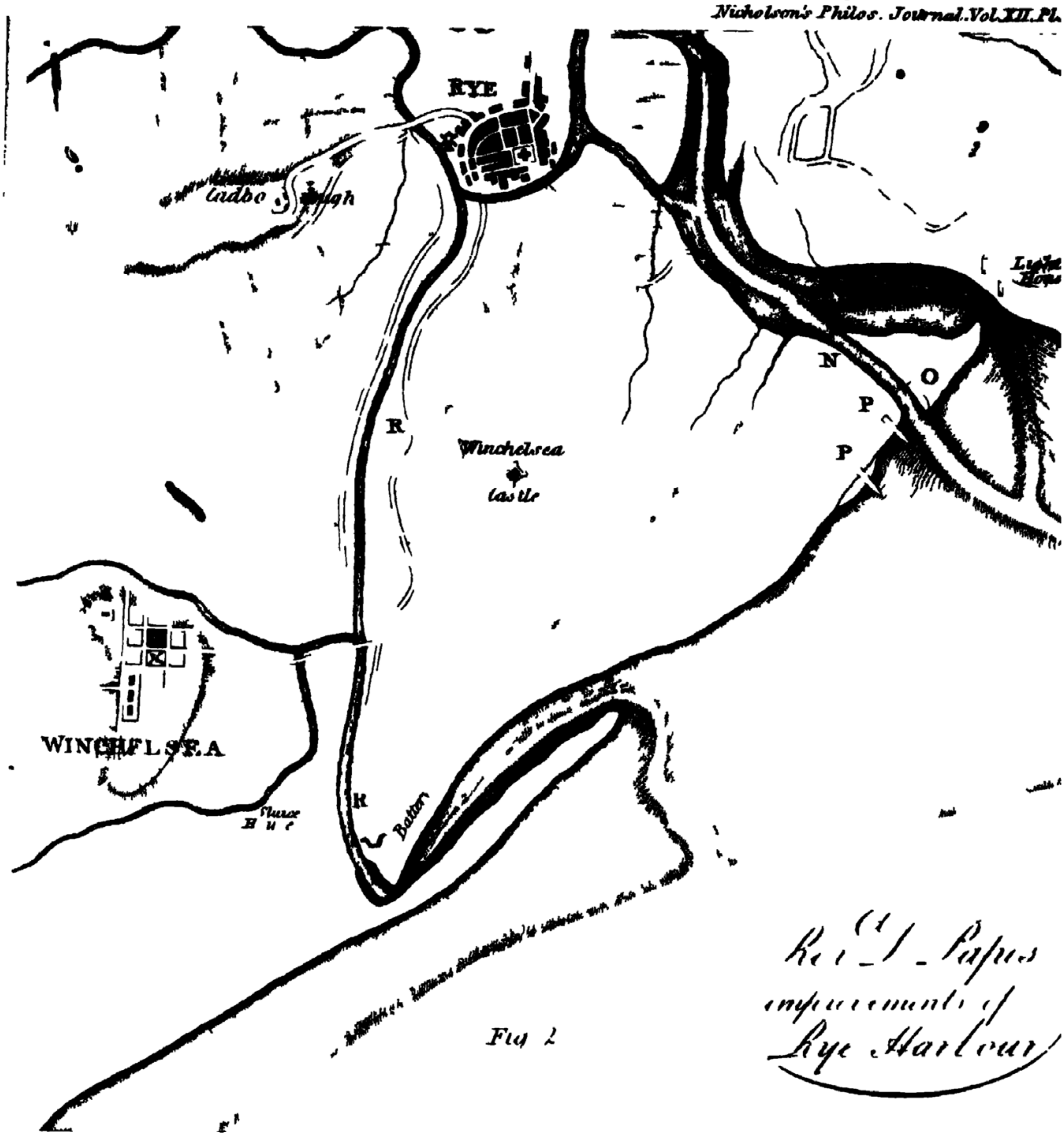
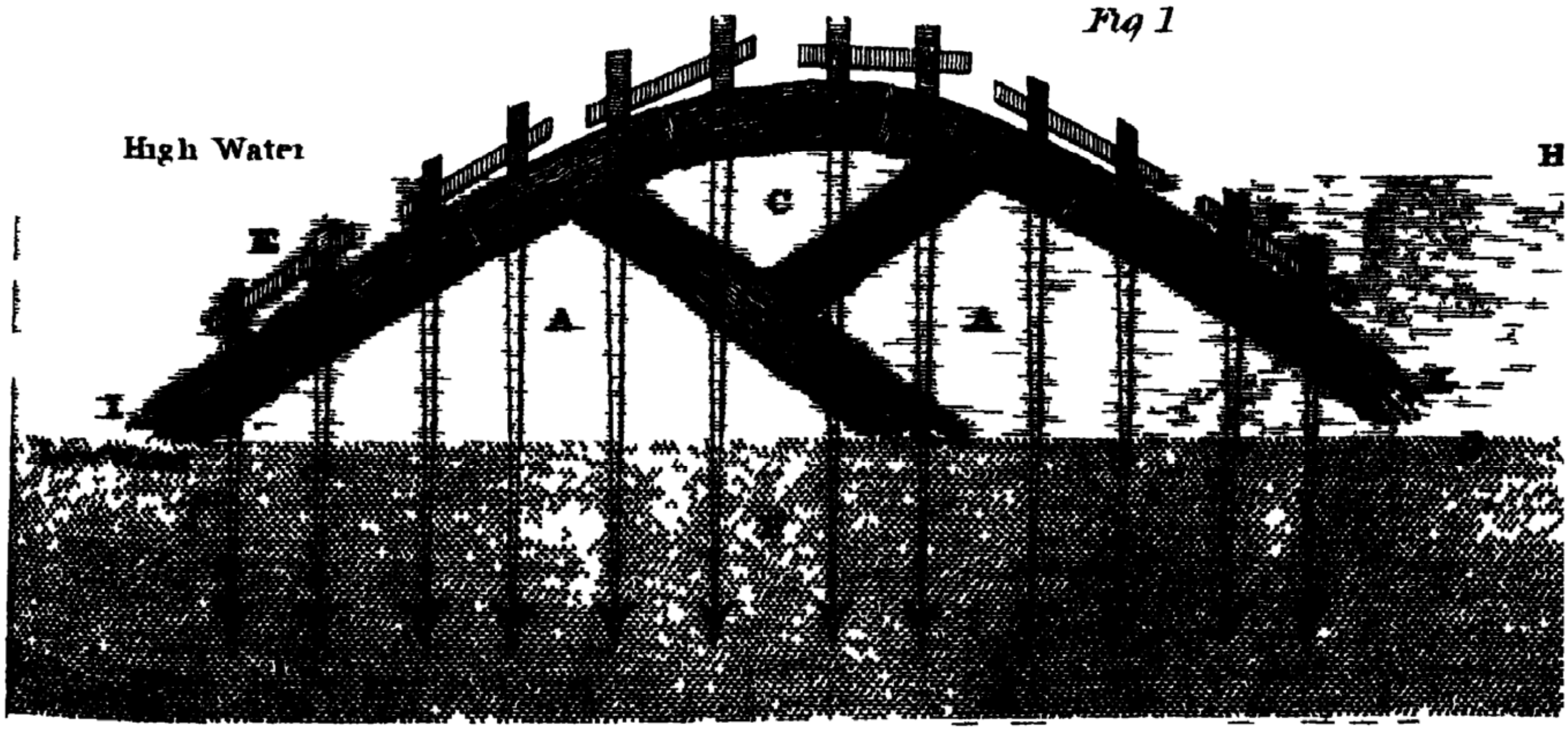


Fig 2

Rev. J. Pape's improvements of Rye Harbour

Cross Section of Mr Pape's Dam

Fig 1



receiver and barrel, and the air by its elastic force rushes into the barrel and fills it. The piston now returns towards the duct B, and drives before it the air contained in the barrel, together with the spring piston N, until they are stopped by the shoulder D at the instant in which the ends of the two pistons come against the middle of the duct B. By forcing the air out at the duct B, the pistons come into contact, and form one uniform cylinder, that prevents any communication of the barrel with the external air. The piston P is now drawn back toward the duct A, and the spring piston N, by the action of its spring, follows in close contact with it, until it is stopped by its shoulder C meeting with the end of the barrel, after having passed the duct B, and having continued to intercept the communication between the barrel and the external air. This is the situation with which the description began; and, repeating the operation, when the piston P is drawn back beyond the duct A, the air from the receiver rushes into the barrel; and when it moves forward to the duct B this air is expelled.

The latter is closed by a spring piston.

Having exhibited a general description of this pump, with the manner of its working, a more particular illustration of some of its parts will be given.

When the pump is intended to exhaust, the receiver must be placed over the duct A, leaving the duct B open to the external air; but when it is designed to condense, nothing more is necessary than to shift the situation of the receiver on the plate, placing it over the duct B, and leaving the duct A open to the external air.

This pump exhausts or condenses at pleasure.

The duct B is continued around the spring piston by means of a circular channel cut into the inside of the barrel, in order that the air might escape from all sides when the pistons come into contact.

It may be observed that all the back space in the barrel between the collar O and the piston P makes a part of the capacity of the receiver; or, to speak more accurately, the space O A between the collar O and the duct A: the space A P between the duct and piston, while it moves from A toward B, being only a temporary dilation of the capacity, and the space A P while it moves from A towards O a temporary contraction of it.

Observations.

For the purpose of preventing a fluctuation of the air in the receiver, which would be caused by this expansion and contraction,

Advantage of constructing it with two barrels.

traction, and might be detrimental in some experiments, the diameter of the duct A is made very small, and another barrel, having similar pistons and ducts, is added, with its rack placed above the pinion wheel, while the other is placed below it. The advantage of a pump of this kind being constructed with two barrels arises from the contrary motions of their pistons; for while one augments the capacity of the receiver by moving forward, the other equally diminishes it by moving backward. An equilibrium is thus maintained that prevents any oscillatory motion in the mercury of the gage, which might arise from the operation of a single barrel.

The resistance from the spring piston.

The additional resistance to be overcome in working this pump, above what is to be met with in other pumps, happens only at the small interval while the spring piston is passing from its natural situation to the duct B. This need not be more than about four times greater than that which is requisite to overcome the friction of the piston P, and will be easily provided or by increasing the proportion between the diameter of the pinion wheel and the sweep of the handle.

ELIZUR WRIGHT, *C. A. S.*

Canaan (Connecticut in America,)
March 12, 1805.

II.

Concerning the State in which the true Sap of Trees is deposited during Winter. By THOMAS ANDREW KNIGHT, *Esq.**

(Concluded from Page 240)

Bulbous and tuberous roots contain the matter that forms leaves,

WE have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms, on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only, as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the

* Phil. Trans. of 1805, p. 97.

matter

matter prepared and depofited in the preceding year; and hence the root becomes exhaufted and fpoiled: and Haffenfratz found that the leaves and flowers and roots of fuch plants afforded no more carbon than he had proved to exift in bulbous roots of the fame weight, whofe leaves and flowers had never expanded.

—and it is highly probable that trees contain the nutriment of their fruits, &c.

As the leaves and flowers of the hyacinth, in the preceding cafe, derived their matter from the bulb, it appears extremely probable that the bloffoms of trees receive their nutriment from the alburnum, particularly as the bloffoms of many fpecies precede their leaves: and, as the roots of plants become weakened and apparently exhaufted, when they have afforded nutriment to a crop of feed, we may fufpect that a tree, which has borne much fruit in one feafon, becomes in a fimilar way exhaufted, and incapable of affording proper nutriment to a crop in the fucceeding year. And I am much inclined to believe that were the wood of a tree in this ftate accurately weighed, it would be found fpecifically lighter than that of a fimilar tree, which had not afforded nutriment to fruit or bloffoms, in the preceding year, or years.

The preparation of this nutriment in the tree implies that the juices circulate.

If it be admitted that the fubftance which enters into the compofition of the firft leaves in the fpring is derived from matter which has undergone fome previous preparation within the plant, (and I am at a lofs to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at leaft,) it muft alfo be admitted that the leaves which are generated in the fummer derive their fubftance from a fimilar fource; and this cannot be conceded without a direct admiffion of the exiftence of vegetable circulation, which is denied by fo many eminent naturalifts. I have not, however, found in their writings a fingle fact to difprove its exiftence, nor any great weight in their arguments, except thofe drawn from two important errors in the admirable works of Hales and Du Hamel, which I have noticed in a former memoir. I fhall therefore proceed to point out the channels, through which I conceive the circulating fluids to pafs.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

When a feed is depofited in the ground, or otherwife expofed to a proper degree of heat and moifture, and expofure to air, water is abforbed by the cotyledons and the young radicle or root is emitted. At this period, and in every fubfequent ftage of the growth of the root, it increafes in length by the addition of new parts to its apex, or point, and not by any general dif-

tenfion

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

tension of its vessels and fibres; and the experiments of Bonnet and Du Hamel leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured infusions, when its point was taken off, and I was totally unable to discover any alburnous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former Paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into and possibly give existence to, other leaves; and thus by a repetition of the same

process

process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

But it has been proved by Du Hamel that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude that when the true sap has been delivered from the cotyledon or leaf into the returning, or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

through the leaf in the preceding year; but that it is generated by a proceſs ſimilar to that of the germination of ſeeds, and that the ſame proceſs is always going forward during the ſpring and ſummer, as long as the tree continues to generate new organs. But towards the concluſion of the ſummer I conceive that the true ſap ſimply accumulates in the alburnum, and thus adds to the ſpecific gravity of winter-felled wood, and increaſes the quantity of its extractive matter.

I have ſome reaſons to believe that the true ſap deſcends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the ſpring, and ſuch trees be ſuffered to grow till the following winter, the alburnum acquires a great degree of hardneſs and durability. If ſubſequent experiments prove that the true ſap deſcends through the alburnum, it will be eaſy to point out the cauſe why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why ſome portion of alburnous matter is in all trees* generated below inciſions through the bark.

It was my intention this year to have troubled you with ſome obſervations on the reproduction of the buds and roots of trees; but as the ſubject of the Paper, which I have now the honour to addreſs to you, appeared to be of more importance, I have deferred thoſe obſervations to a future opportunity; and I ſhall at preſent only obſerve, that I conceive myſelf to be in poſſeſſion of facts to prove that both buds and roots originate from the alburnous ſubſtance of plants, and not, as is, I believe, generally ſuppoſed, from the bark.

I am, &c.

T. ANDREW KNIGHT.

Elton, Dec. 4, 1804.

* I have in a former paper ſtated that the perpendicular ſhoots of the vine form an exception. I ſpoke on the authority of numerous experiments; but they had been made late in the ſummer; and on repeating the ſame experiments at an earlier period, I found the reſult in conformity with my experiments on other trees.

Singular

III.

Singular Method of forming Walls and Roofs of Rural Building, in Indostan, communicated by M. Legoux de Flaix, *Officer of Engineers* *.

THE method which the Indians have used for many years, of forming their rural buildings, unites solidity, convenience, and wholesomeness to economy, and facility of execution. Advantages of this method of building.

Houses constructed in this manner have also the advantage of being absolutely safe from conflagration, and of resisting even the most violent inundations. Resists fire and inundations.

In a country where stone is scarce, the rich build their houses with bricks, which in many respects are preferable to stone; but poor people, such as those employed in agriculture, cannot go to that expence, even in India, where labour and materials are so cheap.

The habitations of villagers in most parts of the globe are built with earth walls, in India they are likewise covered with terraces of earth, and it is evident, that buildings formed with both walls and roofs of earth must necessarily be free from danger of fire.

To prove that buildings of this construction are equally safe from inundations, it is sufficient to state, that on the banks of the Ganges and Indus, (rivers of vast magnitude both in their extent and their course, and whose great bodies of water cause the most destructive effects in their floods,) these houses stand uninjured, though sometimes isolated in the midst of immense inundations for fifteen or twenty days. It is extremely probable, that houses built of stone or brick would not stand this great force of water equally well.

To form houses in this manner, the foundations of the outside and partition walls are dug up, which are sometimes from five to seven feet deep, and always proportioned to the heighth intended to be given to the walls. The excavated earth is exposed till it becomes perfectly dry; if it is of a fat or argillaceous nature, it is carried to a place, prepared for the purpose, where it may be pounded into a dust, and properly prepared for use; when in this state, it is mixed with a third The earth dug from the foundations is pounded fine, and mixed with coarse sand or fine gravel,

* Sonninus's Journal. II. 394.

third or an half of coarse sand, or small gravel passed through a sieve to clear it from pebbles. The fat earth is mixed with the sand and gravel, and worked up well with it, so that the mass may be of an uniform consistence. It is then moistened with water five or six hours before it is wanted, and in the quantity necessary for a single day's work alone.

then moistened with water.

The walls are raised all together,

The mixture thus prepared is carried to the place of building, when the foundations are perfectly dry, and the walls are then built equally in every part at the same time, on a perfect level, in courses, and brought up perpendicularly: each course of earth is from eight to ten inches in depth, and the whole breadth of the wall, which is seldom less than two feet thick, and never exceeds four; which dimensions are always regulated by the intended height of the building, and the force of the floods, if it is near the river. When the walls are three feet and a half, or four feet thick, only one course is raised in a day; but when they are from two feet and half to three feet thick, two courses are raised, and if they are but two feet thick, three courses are sometimes raised in that space of time. This depends on the quickness of the desiccation of the walls, which speedily takes place there, where the dryness of the air is extreme: this would not perhaps happen in our moist climates; if this method of building should be tried here, it would probably be necessary to leave them longer to dry, in order to obtain the requisite tenacity.

from two to four feet thick.

In one two or three courses in a day according to the thickness.

Spaces left for beams, doors, and windows.

When the walls are built to the height for the roof, the proper openings are made for the beams and joists. It is almost needless to add, that the apertures necessary for the doors and windows are made while the walls are building.

The walls when dry are enclosed in open work cases of bambou,

On the twelfth or fifteenth day, or when the walls are sufficiently dry, or to the same degree to which tiles are dried, the walls are surrounded externally and internally with a sort of open work case, made of spars of bambou, or of some other hard and dry wood. In Indostan, where this method of building is general, the workman have bars of iron, which they hire out, that serve to sustain the coffer work mentioned, and are placed at every three or four yards; the coffer work is raised at two or three feet distance from the surface of the walls, according to their thickness, and the space between is filled up with firewood, turfs, and cakes made of cow and sheep dung worked together and dried in the sun.

at two or three feet distance, and the interval filled with fuel.

This

This pile of combuſtibles is arranged in ſeveral ſtages, of three, four, or five feet thick, ſeparated from each other by layers of earth or half dried turf of from eight to ten inches depth; the upper ſtages are firſt ſet on fire, ſo that the wall is baked through its whole extent from top to bottom. The charge of the combuſtibles for each of the ſtages is ſo managed, that the loweſt is the greateſt, and is diminiſhed for each as it is nearer the top of the wall; as the pile burns down the fire of the lower ſtages ſtill acts on the upper part of the walls, which permits the upper ſtages to be of leſs thickneſs. The fire bakes the walls to a thickneſs of from ſix to ten inches, as tiles are baked in a kiln. And thus walls are built in a ſingle piece, and of the greateſt ſolidity, which have the more ſtrength, as there are no junctures in them. Wherefore they ought to prevent the greateſt poſſible reſiſtance to the action of the atmoſphere, the attacks of floods, and the fall of rain, which deſcends in torrents in moſt countries of Indoſtan during the rainy ſeaſon.

Arranged in ſtages ſeparated by layers of earth.

The upper ſtages firſt kindled.

Walls thus baked to the thickneſs of from ſix to ten inches.

Experience has conſtantly proved, that the houſes built in this manner not only laſt much longer than thoſe built of bricks, but that they alſo reſiſt better the attacks of the periodical inundations, and thoſe of the annual rains to which they are expoſed in this climate.

Theſe houſes laſt longer than thoſe of brick, and reſiſt floods and rain better.

The method in which the terrace roofs (which are called in India *argamace*) are formed for theſe houſes, is the following:

Their roofs are made of clay in three layers.

Immediately after the baking and cooling of the walls, the aſhes and the bars which ſuſtained the coffer work are removed. The beams and joiſts are placed, and covered either with very thin boards, or elſe with ſmall green branches; and upon this ſupport the different layers of the terrace roof are placed. The firſt layer is ſimply clay, with an equal quantity of *ole*, a ſpecies of marl in powder, which is pounded in troughs, ſuch as are uſed for preparing mortar. This firſt layer is four or five inches thick, and it is then levelled, and is moiſtened from time to time, in order to beat it firm with ſmall bats. As ſoon as this is dry, the ſecond layer is laid on, which conſiſts of potters clay worked up in the ſame manner as if prepared for making pottery; this layer is only two or three inches thick at moſt. It is levelled according to the ſlope of the terrace, which is given it in placing the beams and

Firſt layer common clay and *ole*, a kind of marl, four or five inches thick.

Second layer potters clay, two or three inches thick.

and joists, and it is consolidated by light blows of wooden trowels, until it is perfectly dry. When the clay forms cracks in drying they are closed by other clay prepared for filling up these chinks to the bottoms.

Third layer clay with one fourth brick dust and one fourth fine sand 6 or 8 inches thick.

The second layer when perfectly dry, and free from cracks, is covered with a third layer; which is composed of pulverized clay mixed with a fourth of brick dust, passed through a close sieve, and with a fourth of fine sand. This mixture is worked up in a trough like mortar; it is used as soon as prepared, and is then spread out equally over the whole terrace six or eight inches thick; this layer is consolidated in the same manner as the others, and this labour is continued till it is perfectly dry; and then the *argamace* is finished. This terrace is strong, and has such tenacity that the most violent rains cannot penetrate it.

Houses thus built cost 6 franks for the cubic fathom.

A building of this species costs in India but six francs (five shillings) for the cubic fathom and is entirely performed by masons. In France it would cost three times as much (and something more in England) on account of the greater expence of labour and fuel.

May be made with many stories.

Houses may be built in this manner of any height required, and of as many stories as are thought fit; I have seen some that had but one ground floor; but I have also seen others that were elevated two stories above the ground floor. One of this last sort, situated on the banks of the Gemna in the province of Alabad, was built above 430 years, and the walls, and the whole of the building looked as fresh as if they were new.

An house of this sort 430 years built seemed quite fresh.

IV.

Account of some new Improvements on Steam-Engines. By Mr. ARTHUR WOOLF.

(Concluded from page 296.)

Mr. Woolf's improvements in steam-engines.

"I have found out and invented a contrivance, by which the temperature of the steam vessel or working cylinder of a steam-engine, or of the steam vessels or cylinders where more than one are used, may be raised to any required temperature, without admitting steam from the boiler into any surrounding receptacle, whether known by the name of a steam case, or by any other denomination. That is to say, instead of admitting steam

steam of a high temperature into such receptacle or steam case, which is always attended with a risk of explosion proportioned to the elasticity of the steam employed, I put into the said surrounding receptacle, or case, oil or the fat of animals, or wax or other substances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour: or I put into the said case or cases mercury or mixtures of metals, as of tin, bismuth, and lead, capable of being kept in a state of fusion in a lower temperature than that intended to be employed in working the steam-engine; and I so form the surrounding case or cases as to make it or them admit the aforesaid oil, or other substance employed, to come into contact not only with the sides of the steam vessel or vessels, or working cylinder or cylinders, but also with the bottom and top of the same, so that the whole may be as much as possible maintained in one uniform temperature; and this temperature I keep up by a fire immediately under or round the case or cases that contains the aforesaid oil or other substance, or by connecting the said case or cases with a separate vessel or vessels, kept at a proper temperature, filled with the oil or other substance made use of as aforesaid. In some circumstances, or whenever the same may be convenient or desirable, I employ the fluid metals, or mixtures of metals, and oil or other of the substances before enumerated, at one and the same time in the same engine; that is to say, in the part of the case or vessel exposed to the greatest action of the fire, I sometimes have the aforesaid metals or mixtures of metals, and in the parts less exposed to the action of the fire, I put oil, or other substances capable of bearing the requisite heat without being converted into vapour.

Mr. Woolf's improvements in steam-engines.

"By this arrangement, and method of applying the surrounding heat, I not only obviate the necessity of employing steam of a great expansive force round the steam vessel or vessels, or the working cylinder or cylinders, as already mentioned, to maintain them at the temperature required, but I am enabled to obtain from steam of a comparatively low temperature, or even from water itself, admitted into the steam vessel or vessels, all the effects that can be obtained from steam of a high temperature, without any of the

Mr. Woolf's improvements in steam-engines.

the risk with which the production of the latter is accompanied, not only to the boiler and other parts of the machinery, but even to the lives of the workmen; for such low steam, or even water, (but in every case steam is preferable,) being admitted into a steam vessel or vessels, or working cylinder or cylinders, kept at the requisite higher temperature by the forementioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine which cannot otherwise be obtained but at a greater expense of fuel, or with the risk of an explosion. By this means I can make use of steam expanded in any required ratio, or of any given temperature, without the necessity of ever having the steam of any greater elasticity than equal to the pressure of the common atmosphere.

" Another improvement which I make use of in steam-engines consists in a method of preventing, as much as possible, the passage of any of the steam from that side of the piston which is acted upon by the said steam to the other side which is open to the condenser; and this I effect, in those steam-engines known by the name of double engines, by employing upon or above the piston mercury or fluid metal, or metals in an altitude equal to the pressure of the steam. The efficacy of this arrangement will appear obvious, from attending to what must take place in working such a piston. When the piston is ascending, that is, when the steam is admitted below the piston, the space on its other side being open to the condenser, the steam endeavouring to pass up by the side of the piston is met and effectually prevented by the column of metal equal or superior to it in pressure, and during the down stroke no steam can possibly pass without first forcing all the metal through. In working what is called a single engine a less considerable altitude of metal is required, because the steam always acts on the upper side of the piston. For single engines, oil or wax, or fat of animals, or similar substances, in sufficient quantity, will answer the purpose, if another improvement, which constitutes part of my said invention, be applied to the engine, namely to take care that in either the double or single engine so to be worked, the outlet that conveys the steam to the condenser shall be so posited, and of such a size, that the steam may pass without forcing

forcing before it or carrying with it any of the metal or other substance employed, that may have passed by the piston; taking care at the same to provide another exit for the metal or other substance collected at the bottom of the steam vessel or working cylinder to convey the same into a reservoir kept at a proper heat, whence it is to be conveyed to the upper side of the piston by a small pump worked by the engine or by any other contrivance. In order that the fluid metal or metals used with the piston may not be oxidated, I always keep some oil or other fluid substance on its surface, to prevent its coming in contact with the atmosphere; and to prevent the necessity of employing a large quantity of fluid metal, I generally make my piston of the depth of the column required, but of a diameter a little less than the steam vessel or working cylinder, excepting where the packing or other fitting is necessary to be applied; so that, in fact, the column of fluid metal forms only a thin body round the piston. In some cases I make a hollow metallic piston, and apply an altitude of fluid metal in the inside of the working cylinder.

Mr. Woolf's improvements in steam-engines.

"It may be necessary, however, to state, that in applying my improved method of keeping the steam vessels of steam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a separate condenser has been introduced, I sometimes employ oil (or any other substance lighter than water, and capable of being kept fluid in the temperature employed, without being converted into vapour,) in the upper part of the tube or pipe attached to the steam vessel; by which means steam of any temperature may be used without being exposed to the risk of partial condensation by the admission of any colder body into the steam vessel; for the oil, or other substance employed for this purpose, soon acquires the requisite temperature; and to prevent unnecessary escape of heat, I construct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the steam vessel which may not be heated exteriorly. And further, (as is already the practice in some engines, and therefore not exclusively claimed by me,) I cause the water raised by the engine to pass off through another ascending tube than the one attached to the steam vessel, but connected with it at some part lower than the oil or other substance employed in it is ever suffered to descend to in the working

working of the engine. The improvement which I have just mentioned, of introducing oil into the pipe attached to the steam vessel of such engines, may also be introduced without applying heat externally to the steam vessel; but in this case part of the effect which would otherwise be gained is lost."

V.

On the Magnesian Earth of Baudissero. By M. GIOBERT.

(Concluded from Page 284.)

The earth of Baudissero analyzed.

TO ascertain the proportions of these constituent parts of the magnesian earth, we lixiviated a given weight of it, and precipitated the sulphuric acid from one part of it by acetite of barytes, and the lime from the other part by oxalate of ammonia.

It contains, besides magnesia, sulphate of lime,

The weight of the oxalate of lime, and that of the sulphate of barytes, obtained from it, shewed us that it contained 1,60 of the sulphate of lime. The experiments before recited determine the proportion of the silex contained.

—with silex,

—and carbonic acid,

To prove that of the carbonic acid, we both calcined a given weight of the earth in crucibles, from which syphons passed into bottles containing lime water; in order that the carbonic acid gas furnished by the earth might be precipitated, and also dissolved considerable quantities of it in acids by the action of heat, and received the gas produced in bottles filled in like manner with lime water; the first method produced constantly the most. The carbonate of lime formed in these different experiments apprized us that 100 parts of the earth contained from 8 to 12 of carbonic acid, and sometimes a little less in the stony species.

—and water.

If this weight of the carbonic acid be deducted from the loss of weight which this earth suffers by the calcination in the fire, which was mentioned before, we shall then have the quantity of water which the earth contains. In collecting the results of the different experiments, it appears that the earth of Baudissero is composed of

Magnesia

Proportions tabulated.

Magnesia - - -	68
Carbonic acid - -	12
Silex - - -	15,60
Sulphate of lime - -	1,60
Water - - -	3
	100,20

It is from these results that I denominate this earth native magnesia. It is doubtless found mixed with a little silex; but if the title of native alumen is given to the alumenous earth of Halle in Saxony, which contains 24 parts of the sulphate of lime; if the name of native magnesia is given to that of Moravia, announced by Mitchael, of which 100 parts contain 50 of carbonic acid; it appears to me that the earth which I describe has a much better title to the name which I have given it.

It may be called with propriety native magnesia.

The earth of Baudissero affords a subject for interesting observations in the investigation of its origin. Many facts lead me to believe that this earth and the Corneen stone or Cacholong, described and analysed by my colleague Bonvoisin, are both of the same nature. It appears to me that Cacholong at a certain point of its decomposition forms what Bonvoisin calls the hydrophane of Piedmont, and that in its complete decomposition it forms the magnesian earth of which I here give the analysis. Bonvoisin has declared himself of an opinion precisely contrary to this; for he has supposed that this earth, far from being the product of the decomposition of Cacholong, is the element of its formation. Our colleague Gioanetti is of the same opinion. In these two hypotheses, the change of one earth into another is manifest, that is to say, the change of silex and alumen into magnesia in my method of considering the matter; (for it is principally of these two earths that Cacholong and Hydrophane are composed, from the analysis of Bonvoisin;) and the change of magnesia into alumen and silex according to the hypothesis of Bonvoisin and Gioanetti. As this subject appears to me to be very interesting, I intend to make a comparative analysis of these stones at the different degrees of their decomposition or entering into the state of agate *(agatisation)* which shall be the subject of another memoir.

Supposed to be produced by the decomposition of the Corneen stone or Cacholong.

And therefore that either alumen and silex is changed into magnesia, —or the converse.

 There

There remains yet for me to examine the economical uses for which this earth may be employed.

The experiment which I related in the beginning of this memoir, of the decompofition of the fulphate of iron by this earth, which produced an excellent fulphate of magnefia, indicates one of the methods in which it might be ufed to advantage.

Sulphate of magnefia may be manufactured from it to advantage by the procefs with fulphate of iron, defcribed before, more pure than that of commerce in general.

Twenty-five pounds of fulphate of iron coft only three francs with us, while the price of the fame weight of fulphate of magnefia is eight francs, from this it follows that this procefs may be followed to advantage. To this may be added that the fulphate of magnefia of commerce, being impure, and mixed with much fulphate of foda, cannot be compared to that which may be procured in this manner, which equals the beft falt from * * canal; fo that in this comparifon the more pure fulphate of magnefia thus obtained, may be valued at ten francs at leaft, and in reality is worth more.

This however is not the beft method to purfue, when the operator has it in his power to follow the others, which I am going to recite.

The following experiments make known two proceffes much more economical.

In the firft experiment I took two pounds of the earth of Baudiffero, reduced to a coarfe powder, with the fame quantity of the fulphuret of iron of Brozo reduced to powder in like manner, I mixed them together carefully, and treated one half in a crucible on the fire, and the other half in an iron capfule.

Or by pounding thefe and heating to rednefs in crucibles:

In both the mixture heated to rednefs emitted fparks, efpecially on being ftirred. It feemed to become reduced to a very fine powder; a fort of boiling took place, produced doubtlefsly by the difengagement of carbonic acid, and here and there appeared flames of fulphur, which burned without exhibiting any fign of the production of a fulphuret. The fulphurous odour was not however very troublefome, from whence it appeared that the magnefia abforbed with readinefs the fulphuric and fulphurous acids in proportion as they were formed by combuftion. The mixture became of a blackifh grey, or more properly a black; but which appeared grey from the white particles which ftill remained mixed with it.

After

After being left three hours to cool, it was moiſtened with water and put away till next day, a part of it was then lixiviated; the ſolution being made clear and treated with ammonia, gave an abundant and very white precipitate. This circumſtance indicating that much of the magneſia was combined with ſulphuric acid in the operation, all the remainder was lixiviated. The very clear lixivium, evaporated properly, produced at the firſt cryſtalization a pound of ſulphate of magneſia in beautiful cryſtals. The remaining liquor gave on ſucceſſive evaporations a pound and half more of the ſame ſalt in fine cryſtals, very dry and very white. The liquor produced cryſtals to the laſt drop, and the mother-water never became foul.

Leaving to digeſt ſome time, moiſtened with cold water, Lixiviating, and cryſtalizing, fine cryſtals in abundance are had.

The mixture which remained after lixiviation was roaſted a ſecond time, and again produced ſulphate of magneſia: It was then thrown away, although apparently it would have yielded more ſulphate of magneſia after another torrification.

The mother-water, evaporated ſucceſſively gives cryſtals to the laſt drop. The reſiduum roaſted again and re-lixiviated gives more cryſtals.

In another experiment, pure ſulphur was uſed inſtead of the pyrites; it was eaſy to foreſee that the reſult would be the ſame; it was however deſirable to prove it; and the reſult was perfectly ſatisfactory.

Pure ſulphur uſed in place of the ſulphate of iron anſwers equally well.

The uſe then which may be made of this earth, conſiſts in forming with it ſulphate of magneſia. The means by which this may be done are perhaps the moſt ſimple poſſible. It is ſufficient to reduce to powder the earth and the ſulphur, or the ſulphuret of iron, where it can be eaſily procured, as may be done at Baudiſſero. Theſe ſubſtances ſhould be mixed in almoſt equal parts; for it is uſeful to proceed with an exceſs of the earth, and the more ſo, as its coſt is almoſt nothing: The mixture ſhould be torrified in an oven or kiln, heated to the degree at which ſulphur inflames, and when there appear no more jets of ſulphurous flames, the kiln is to be left to cool. The matter being then drawn out ſhould be moiſtened with water in ciſterns, and left for ſome days, only taking care to ſtir it in that time.

Proceſs for ſulphate of magneſia in the large way with magneſian earth and pyrites, ſimilar to the foregoing.

The part of the ſulphur which in burning had only paſſed to the ſtate of ſulphurous acid, oxigenates gradually, or the ſalt, which at firſt was but a ſulphite changes to a ſulphate. The matter is then to be lixiviated, in the ſame manner that is uſed for nitrous earths, the liquor ſufficiently evaporated, and left to cryſtalize by cooling.

Another

Another process in the large way, where pyrites are burned. The kiln may be covered with heap of magnesian earth.

Another method may be followed in places where sulphurets are worked; or where, as at Brozo, there is a manufacture of sulphate of iron. The kiln, where the pyrites are burned, may be covered with an heap of the magnesian earth; the sulphuric acid, which is disengaged will be absorbed by the magnesia; and to the advantage of putting an end to the complaints of the owners of property near the manufacture, will be added that of *sulphating* the magnesia, from which the salt may afterwards be procured by lixiviation. This last process, if it were introduced into the manufactory of Brozo, would produce the sulphate of magnesia of commerce at a very moderate price.

Which when *sulphated* by the acid vapours, may be lixiviated.

The magnesian earth useful for porcelain and pottery,

As the magnesian earth of Baudissero forms an excellent porcelain with silex, it presents besides an interesting subject for research relative to the fabrication of pottery. With this earth and a quantity of the argillaceous earth of Castellamonte sufficient to unite it into a paste, I formed some crucibles and capsules. These crucibles were exposed for 48 hours in the furnace of the glass-house of Po. The earths did not seem to have formed a sufficient union; nevertheless the hardness of the crucibles was such that they could not be affected by the file. Doctor Gioanetti, who is now engaged in manufacture of stone-ware pottery, will hereafter throw light on this subject.

and mixed with argil forms crucibles extremely hard.

I end this part of my memoir with observing that the trials which I made of this earth as an absorbent in veterinary medicine succeeded perfectly well.

Additions to the preceding Memoir, by the same.

The earth of Castellamonte is similar to that of Baudissero.

Farther researches which I made on argillaceous earths have given me to understand that the earth of Baudissero is not the only one known that consists for the most part of magnesia. The same kind is also found at Castellamonte, a large village near that of Baudissero.

M. Bertoline, doctor of medicine, one of the most eminent of my pupils, having repeated the detail of the experiments which we made at the general school of chemistry, invited us to essay a particular earth of Castellamonte, his country, which he thought would furnish the alumen which was sought for unsuccessfully in the earth of Baudissero; soon after, by the care of M. Onorato, surgeon of Castellamonte, who is the pro-

prie-

prietor of the land where this earth is found, I received a large quantity of it, and we examined it comparatively with that of Baudiſſero.

The earth of Caſtellamonte, which was brought to us had nearly the ſame appearance as that of Baudiſſero; but when it is firſt dug up from the ground, it has on the other hand different external appearances, which ſeem to depend on the different degrees of decompoſition of the Corneen ſtone or Cacholong, which is found at Caſtellamonte as well as at Baudiſſero.

When firſt dug up appears different in colour.

The colour of this earth is a white inclining to blueiſh. In a maſs this earth is opaque; but when ſmall fragments of it of a minute thickneſs are examined, they have a ſemi-tranſparency.

In this reſpect it has a ſtrong reſemblance to horn; it is very ſoft, and may be cut with a knife like hard cheeſe. It is more unctuous to the touch, and a little more adheſive to the tongue than the earth of Baudiſſero.

Reſembles horn, cuts like hard cheeſe, more unctuous, and adheres more to the tongue than the firſt.

Treated with the acids, like the ſoft ſpecies of Baudiſſero, it becomes diluted, and then diſſolves, but has however a very remarkable difference, which is that it diſſolves in all the acids without the leaſt effervescence.

Does not effervesce with acids.

It alſo does not yield the leaſt appearance of carbonic acid on expoſing it to the fire in cloſed veſſels furniſhed with ſyphons, which communicate with lime water.

This earth, like that of Baudiſſero, does not contain the leaſt trace of alumine or of oxide of iron.

It contains, ſimilarly to that of Baudiſſero, a little ſulphate of lime and muriate of magneſia, which may be ſeparated by lixiviation in water.

Conſiſts of the ſame ſubſtances as the firſt.

The remainder conſiſts entirely of magneſia and ſilex; but the proportion of this laſt is greater in it than in that of Baudiſſero. It may be computed at from 10 to 20 hundredth parts.

But contains more ſilex.

When this earth is kept in contact with the air its external characters change.

Changes its appearance on expoſure to the air,

Its colour becomes by degrees a dull white, the ſame as has been remarked of the earth of Baudiſſero.

to a dull white;

Its ſemi-tranſparence is loſt; its particles ſeparate, and in two or three weeks it is found to have abſorbed carbonic acid to that degree as to make as marked an efferveſcence with the acid

Loſes its ſemi-tranſparence, and abſorbs carbonic acid, ſo as to effervese with acids, and be-

comes like the earth of Baudisfero, with the small difference noticed.

acid as the earth of Baudissero. In a word, it is completely the same as this last, with this sole difference, that physically considered it is less compact, and becomes even friable, and chemically considered it contains a little more silex.

It appears then to be well proved that the earth of Baudissero and that of Castellamonte are each a true native magnesia, mixed with a little silex. In the earth of Castellamonte it is sufficiently demonstrated that it contains no carbonic acid when in the bosom of the earth; and that it only contains it when, after a long exposure to contact with the air, it can absorb it from the atmosphere. That of Baudissero contains in truth carbonic acid, but the quantity is much inferior to that which it ought necessarily to contain to be considered a carbonate of magnesia; besides the earth of Baudissero having been worked for a long time, and being thus in contact with the air, it is from the atmosphere it must have drawn it, and that in proportion to the time it has been exposed; at least I have no doubt that if the earth of Baudissero was dug up from a certain depth, no carbonic acid would be found in it.

Earth of Baudissero has not acid enough to be carbonate of magnesia.

If dug from a sufficient depth would probably contain no carbonic acid.

I will conclude this addition to the memoir, by observing that the earth of Musinet at Caselette, being produced by the decomposition of the same Corneen stone or Cacholong, ought also probably to be a magnesian earth; but I have not yet made any experiments on this earth; Doctor Bonvoisin, who has given the analysis of it in its state of Cacholong and Hydrophane stone, proposes in conjunction with me to repeat the analysis of this stone, in the true state above-mentioned, and in its earthy state; which shall be the subject of a particular memoir. *

Earth of Caselette probably magnesian also.

The author proposes to analyse Cacholong and Hydrophane, and write a memoir of the result.

* The last use which Mr. Giobert mentions for magnesian earth is of the most consequence of the two, for as sulphate of magnesia is only used in medicine, the sale could not be sufficiently extensive to produce much profit on a large scale.

The use of this earth for pottery is the more deserving of notice, as it has hitherto been supposed that argil was alone proper for this purpose; and though it was long known that magnesia is of a very refractory nature in the fire, Mr. Giobert seems to be the first who thought of using it in crucibles; which is the more extraordinary, as the *lapis ollaris*, which derives its name from its property of serving to make utensils to bear the fire, is well known to contain a large proportion of magnesia.

VI.

A Communication on an artificial Tan prepared from Coal, charred Wood, resinous Substances, &c. Abridged from the Original of CHARLES HATCHETT, *Esq. F. R. S.**

HATCHETT first notices, that the natural tannin was first extracted from the matters which contain it, by Mr. Deyeux, who considered it as a species of resin; that Mr. Seguin first discovered it to be the substance which in the process of tanning renders animal skins insoluble in water, and imputrescible; but that Mr. Chenevix alone had noticed the effect of heat in giving coffee berries the power in decoction of precipitating gelaten. He then states, that his experiments on lac, and some of the resins having shewed him the powerful action of nitric acid on such substances, induced him to try its effect on asphaltum and jet; these with it formed a dark brown solution, and a precipitate, which by digestion in another portion of the acid became dissolved, and on evaporation produced a yellow viscid substance soluble in water and alcohol, and perfectly similar to that obtained by similar means from the resins, excepting that when burned it had the colour of the fat oils. This result led to the supposition, that the dark brown solution was of the carbonaceous

First discovery of tannin by M. Seguin and Deyeux.

Result of experiments on solution of lac, induced others on asphaltum and jet, with nitric acid.

As native magnesian earth would doubtless be of great use to the potteries of this country, it is a pleasing consideration, that it is extremely probable it may be found in England, as well as on the Continent; for not only steatites and other magnesian stones have already been discovered here, but that salt, which it is M. Giobert's principal object to manufacture, is the natural produce of this country, and therefore the neighbourhood of Epsom, which gives it its name, may well be suspected of containing beds of an earth similar to that of Baudissero.

There is also some reason to suppose, that this earth may be one of those ingredients in china-ware, which the Chinese endeavour to keep secret; indeed it is hardly probable they should be ignorant of its use, in a country, where the finest earthern-ware has been manufactured in the greatest perfection, from periods antecedent to the dates of the authenticated history of Europe, and where of course experiments relative to the composition of this article, must have been varied to the greatest extent.—B.

* Philos. Trans. 1805.

matter

matter, and that the yellow precipitate was the effential part of the bitumens, which was confirmed by refults from amber; feveral experiments were tried with various forts of pit-coal, from all which the brown folution was obtained in abundance, but thofe which contained little or no bitumen did not yield the yellow precipitate.

Pit-coal treated in the fame manner,

Procefs with the Coal.

100 grains of coal, in each experiment, were digefted in an open matrafs in a fand heat, with an ounce of nitric acid (of the fp. gravity of 1.40) diluted with two ounces of water, which when warm produced effervefcence, and difcharged much gas; after two days, a fecond, and fometimes a third ounce of the acid was added, and the digeftion continued for five or fix days, when nearly the whole was diffolved, except the precipitate which was conftantly feparated.

and charcoal.

Charcoal was next tried, which diffolved more readily than the pit coal, and left no refiduum.

The feveral folutions from afphaltum, jet, pit coal, and charcoal, were evaporated to drynefs gradually to prevent burning the refidue, which from all were of a gloffy brown fubftance, of a refinous fracture, and had the following properties.

Properties of the refidua of thefe folutions.

1. They were fpeedily diffolved by cold water, or alcohol.
2. Their flavour was aftringent.
3. Expofed to heat, they fwelled much, and gave a bulky coal.
4. Their folutions in water reddened litmus paper.
5. And gave copioufly precipitates from muriate of tin, acetite of lead. oxy-fulphate of iron, of a brown colour, except the tin, which was dark grey.
6. They precipated gold in the metallic ftate from its folution.
7. They alfo precipitated the nitrates of lime, and of barytes, and other earthy falts.
8. The fixed alkalis, and ammonia added at firft, deepened their colour, and afterwards made them turbid.
9. They caufed precipitates from glue or ifinglafs folutions in water, more or lefs brown according to their ftrength, which were foluble in cold and boiling water, fo that in their

effential

effential properties they proved fimilar to thofe formed by the varieties of tannin hitherto known, except that they contained no gallic acid or mucilage.

Animal coal from ifinglafs was alfo tried in the fame manner, this diffolved very flowly, but left a little of the coal unchanged, its folution was of a deeper colour, and managed as the others defcribed, produced fimilar effects with the reagents, except fome difference in the colour of the precipitates; and alfo gave an infoluble precipitate from the folution of ifinglafs; by which the curious fact is proved, that one portion of the fkin of an animal may be made to convert another into leather.

Refiduum of animal coal has all the fame qualities, nearly,

Coak gave a folution refembling that of pit coal, but did not produce the fame yellow precipitate.

and of coak alfo.

Thefe experiments fhew, that the tanning fubftance is beft procured from carbonaceous matter when it is uncombined with any fubftance but oxigen; which was confirmed by experiments on Bovey coal, Suffex coal, Surturband from Iceland, and deal faw-duft, which being diffolved in nitric acid, and evaporated, the refidues diffolved in water, neither precipitated gelaten, or fhewed any other figns of tanning matter; but when the fame materials are charred, and treated as before defcribed, they copioufly produced the artificial tan; as did alfo teak wood, which Mr. Hatchett had proved to contain neither tannin or gallic acid in its uncharred ftate.

Tanning fubftance beft procured from carbon, uncombined with any thing but oxigen.

Mr. Hatchett had made feveral experiments on the flow carbonization of vegetable matters in the humid way, principally by fulphuric acid, occafionally diluted. Concentrated fulphuric acid poured on any refinous fubftance reduced to powder, diffolves it in a few minutes; the folution is tranfparent, of a yellow brown colour, and a vifcid oil-like confiftence, but after being placed on the fand bath, grows darker, evolves fulphuric acid gas, and at laft becomes a thick liquid of an intenfe black.

Carbonization of vegetable matter by fulphuric acid.

Sulphuric acid of the above ftrength poured on common turpentine diffolves it readily, if a portion of the folution is then dropped into cold water, a precipitate of common yellow refin is formed; if after another hour or two, another portion is treated in the fame way, the refin produced is of a dark brown, and that thus formed from a folution that has ftood

Effects of folution of turpentine in fulphuric acid dropped into water.

five

five or six hours, is completely black. When the digestion is continued for several days, until no more gas is given out, the resin will be converted into a black porous coal, which does not contain any resin, if the experiment has been properly conducted. This coal was about 43 *per cent* of the resin used, and after being exposed in a platina crucible loosely covered to a red heat, still amounted to 30 *per cent*, and by the slowness of its combustion, and other circumstances, approached nearly the character of some mineral coals.

Products of this operation dissolved in nitric acid.

A portion of the coal, the black resin, brown resin, and yellow resin obtained from the turpentine described, and also some of the turpentine itself, were each dissolved in nitric acid, and reduced to dryness; the residua, which varied in colour from yellow to dark brown, according to the substance employed, were dissolved in water, and examined with isinglass and other reagents.

Their residues after evaporation are tried with gelaten, &c

The solution from the turpentine residuum, that of the yellow resin, and the brown resin, did not precipitate gelaten.

Effects produced.

That from the black resin yielded a considerable portion of the tanning substance, and that from the coal afforded it in great abundance. Hence it appears, that these substances yielded artificial tan only in proportion to their conversion into carbon.

Various kinds of wood, copal, amber and wax, reduced to coal by sulphuric acid, yielded similar products on being treated with nitric acid.

Tan formed by alcohol.

Mr. Hatchett formed the artificial tan from the resins, and gum resins (such as common resin, elemi, assafœtida, &c.) when reduced to the state of coal from long digestion with sulphuric acid, by means of alcohol, without using any nitric acid: In the carbonized state mentioned, they are digested in the alcohol, a portion is dissolved, a dark brown solution is formed, which by evaporation yields a mass soluble in water as well as in alcohol, and which precipitates gelaten, acetite of lead, and muriate of tin, but produces only a slight effect on oxy-sulphate of iron.

Supposition relative to tan from peat.

The author supposes, that the tanning matter known to be evolved by peat in certain places, is effected by a process in some respects similar to the above, since if it was produced by its mere digestion in water, all peat would afford it, which is contrary to experience.

Mr.

Mr. Hatchett put his discovery to the test of real practice, having actually *converted skins into leather by the artificial tan* procured as described, but observes, that the production of this substance, for the present, must be considered only a curious chemical fact, not altogether unimportant, and not capable of economical application, though he hopes, that hereafter a process may be discovered for preparing this species of tan sufficiently cheap to enable tanners to use it in their business.

Leather made by the artificial tan.

Hope of its economical production.

There is reason to suppose, that it would be superior to common tan for this purpose, as it appears from experiments mentioned in Mr. Hatchett's second paper on the same subject (which will be given in the next number) that "solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c."*

Reason for thinking it superior to common tan.

VII.

Memoir on the Discovery of a Factitious Puzzolana, presented to the French National Institute, by M. DODUN, *Engineer in Chief of Bridges and Highways in France* †.

THE deposited dust of ancient volcanic substances, has been long used in Flanders, and the adjacent countries, as a substitute for the Italian puzzolana, under the name of *trass*, or *ashes of Tournai*.

Factitious puzzolana of Flanders,

M. Faujas has proved by decisive experiments, made by order of government, that certain lutulent eruptions of ancient volcanoes at Vivarais, had the same qualities as the

that of Faujas.

* The peculiar tanning property of the water of certain peat bogs and morasses, may be otherwise accounted for, than by Mr. Hatchett's supposition, by the fact, that peat is not uniformly the production of the same vegetable substances: wherever heath, tormentil, and perhaps some other plants, are found in abundance, the peat water will have this quality; in the case of heath, at least, it cannot be doubted; and perhaps the peat which does not yield tan may owe this deficiency to the total absence of vegetables of this species.—B.

† *Journal de Physique*, Tom. 61.

puzzolana

that of M. Bagge.

puzzolana of Italy, and might be used instead of it. M. Bagge, a Swede, is also known to have composed an artificial puzzolana cement, with a black, hard, and flatey schist; but until 1787, no one ever thought that the territory of France contained in abundance non-volcanic substances capable of taking place of the Italian puzzolana with economy and advantage.

M. Dodun discovered his by chance.

The discovery which I here present has, like many others of great utility, been the effect of chance.

The habit of examining the nature of stone in its bed, which enables the observer to judge of its qualities at first sight, fixed my attention on an immense quantity of calciform fragments of iron ore, in beds of from eight to ten feet thickness, following exactly the parallelism of the slightly inclined declivities, in the neighbourhood of Castlenaudery. I perceived in the adjacent fields many substances of the same nature scattered over the surface of the earth, of violet, brown, and black colours, which from their appearance, had a perfect resemblance to compact lava, which seemed extraordinary in a country where there was no appearance of ancient craters, or of volcanic eruptions. These I soon found out had been brought to this state by serving as hearths, or enclosures to the fires kindled in the fields by the peasants, either for agricultural purposes, or personal convenience when they watch their flocks in winter; as I saw soon after many similar arranged by hand on one another for these purposes.

Saw great beds of ferrigunous oxides at Castelnaudery.

Observed burnt fragments in the fields like compact lava,

which similarity made him think them fit for puzzolana.

The similarity of these fragments to volcanic products excited my desire to form a cement from them, by treating them in the same manner as puzzolana earth. The great quantity of iron which these oxides seemed to me to contain, the abundance of their siliceous particles, and the alumen which evidently entered into their composition, their great weight, and their non-effervescence with acids, altogether made me presume, that the cement formed from them would bind under water, and my expectation was not deceived.

Convinced by long experiments of its superiority to the Italian.

Fifteen months successive experiments, to discover the proportion of lime which this oxide would absorb to harden in water, without cracking when in the air, have convinced me, that my factitious puzzolona had all the good properties of that of Italy, without its faults. At this time I determined to propose its use in the public works, and demanded that comparative experiments should be made between it and the

Proposed for use in the public works.

the Italian puzzolana, in presence of the Commissaries of the Province of Languedoc, and of the Directors of the canal which joins the two seas. Great blocks of *Beton* composition made with both cements, were thrown into the reservoirs adjacent to the lock of Saint Roch, at Castelmandery, being first plaistered over with the respective compositions

and tried comparatively before the Commissaries of Languedoc. In the reservoir near the lock of St. Roch.

Six months after, the water was drawn off from the bodies of masonry, and it was then seen that the factitious puzzalona had acquired a solidity at least equal to that of Italy. The plaister made with the Italian puzzalona was cracked and chapped, but that formed from the factitious kind had entirely preserved the unity of its surface.

Its superiority to the Italian sort.

The states of *Etats* of Languedoc altogether convinced of the authenticity of this discovery, by the results of the comparative trials of both kinds of cement which they had seen, and by the certificates of their commissaries, and persuaded of the great advantage it would be to France, decreed in 1789, in their last meeting, that the factitious puzzolana should not only be used instead of the Italian in the works under their direction; but moreover, that it should be demanded in favour of the author of it, as a testimony of public gratitude, that government should authorize the free circulation of it every where.

Testimony in its favour from the states of Languedoc.

The great consumption of this factitious puzzolana obliged me to extend its manufacture, I formed a partnership with the proprietor of the ground. The foundation of an establishment on a great scale was laid at the mountain itself where the materials were found. The works carried on in its vicinity were likely to farther reduce the cost of the article, which was already one half less than that of Italy, and the public were about to enjoy the advantages of this manufacture, when the revolution paralysed every thing.

Extensive works of it began.

Stopped by the revolution.

In 1791 I informed the constituent assembly of this discovery; the certificates which proved it, and the results of the experiments were deposited at the office; the matter was ordered to be examined by M M. Pelletier and Berthollet, and the assembly considering, that this factitious puzzolana might be of the greatest use to France, decreed that 2000 francs should be granted to its author, which was paid accordingly.

The discovery declared to the constituent assembly.

Approved of and rewarded.

On

On this occasion the celebrated Mirabeau declared the discovery to be so valuable, "*that if it had not yet been made, public encouragement should be held out to excite it.*"

The troubles of France retards the manufacture of it.

The Constituent Assembly wished to have numerous similar establishments set on foot in France, so well were they convinced of its national importance; but the misfortunes of the times prevented the execution of a project, which the grand Chief of the empire may easily realize, to the advantage of the country, whenever it seems good to him to do so.

Researches on the amelioration of our cements, and particularly on the nature of the materials proper to form artificial puzzolana, led me to try the calcination of various schists, of the bitumenous, ferruginous, and argillaceous sorts.

Examination of different schists.

The black slatey schist of M. Bragge, so common in France, was not forgotten: It is almost the same as that which the elder M. Grathieu essayed at Cherbourg last year; but I have constantly found that these schists always contain too little iron. I perceived that their repulsion of the water was slow and feeble, and that their solidification in the water was owing to the good quality of the lime.

Contain too little iron for puzzolana.

I was thus obliged to recur to my quartzose oxides of iron, from their containing a greater quantity of ferruginous principles; and can aver with the skilful Faujas, that the puzzolanas owe their property of hardening in water solely to the ferruginous particles which they contain: of this I have had many proofs. This truth is farther demonstrated in the pudding-stones, the brescias, and generally in all the amygdaloides with a ferruginous base or cement.

Puzzolanas owe their qualities to the iron contained.

Theory of cements little advanced.

The theory of our cements is but little advanced; perhaps we take simple conjectures for proofs relative to them. We effect the regeneration of silex, and of the carbonate of lime; we know the acid gases which perform the principal part in the affair: but in this important work we have been long ignorant of the degrees of their reciprocal affinity, their quantity, and the mode of their respective combinations. Our knowledge on this matter is confined to a few facts.

Two different preparations of the puzzolana.

Many experiments have proved to me that the puzzolana, which soonest forms a body in the water, is not fit to be employed in the open air, where it cracks and chaps in all directions. And that which is proper for the air, and which ac-

quires

quires and preſerves its tenacity in it, ſets but imperfectly in water. This difficulty, of which the Inſtitute will perceive the cauſe, has obliged me to keep two ſorts of the factitious puzzolana; on the reciprocal uſe of which a memoir of inſtruction will accompany the ſale. The two ſorts may be diſtinguiſhed by their colour.

The factitious puzzolana proper for works under water, is of a reddiſh-brown. That which is fit for works expoſed to the air, is a dark violet. The latter is uſed for terraces, the embankments of baſons, for the compoſition of incloſures, or for light roofs. Bridges of a ſingle arch may be formed with it; and I have ſeen it adhere ſo ſtrongly to glazed tiles, that it was neceſſary to break the tiles to detach it.

One fit for water-works.

Another for expoſure to the air, and proper for terraces, roofs, and arches.

The puzzolana proper for conſtructions beneath the water, forms the moſt ſolid body in it. Three months after immerſion it is an actual ſtone capable of receiving a poliſh. The lime in it is always regenerated into carbonate of lime in ten weeks.

Water puzzolana forms a ſtone capable of a poliſh.

When it may be thought by any one that he has been deceived as to the certainty of theſe effects, it will always be found, that he either has not obſerved the quantities directed of the puzzolana and the lime, or that he has uſed the reverſe of that kind of the cement proper for the work.

Nullity of effect cauſed ſolely by miſtakes of the operator.

I commonly uſed lime in the ſtate of impalpable powder, ſlacked in Lafaye's manner, for works expoſed to the air; and employed lime in the ſtate of *putty*, for works which were to be covered with water. Sometimes I uſed lime in powder for the ſame work. This difference depends on the degree of goodneſs of the lime, on its greater or leſſer richneſs, or its proportional poverty. Cuſtom gives the advantage of knowing the different kinds on mere inſpection.

Lime uſed in powder with it; and in putty.

The uſe of lime in powder appeared to me to merit a preference in the preparation of mortars or cements. I prepared my factitious puzzolana in a certain quantity as ſoon as I knew the proper proportion of the lime; and I had thus the advantage of being able to work it in troughs in the ſame manner as ſulphate of lime. The whole was well mixed together and put into ſacks; by which means the maſons had nothing to do with the mixture of the articles (which is too often left to unprincipled workmen); and being thus maſter of the reſpective

Advantages of getting the materials ready mixed.

spective proportions of the puzzolana and the lime, I could always be assured of the solidity of my cements.

Exterior characters of the iron oxides used.

There remains for me to describe the exterior characters of the quartziferous ferruginous oxides, which form the basis of my factitious puzzolana, and to relate the analysis of them which I made about 18 years ago. I will content myself with offering the comparative results with the Italian puzzolana, both in the dry way and the moist.

Exterior Characters of the quartziferous Oxides of Iron.

Slight calcination changes them from brown to red. A greater changes them to a deep brown.

Their colour is of a reddish-brown before calcination, or slightly violet. A light torrification gives them a clearer red tint or a deep violet: one more intense renders them of a deep brown or of a violet-brown inclining to a black. The degree of the calcination for use is confined to those two states.

Long continued heat renders them black and porous like lava.

Urged at a longer continued heat, the colour becomes a deep black, then the substance becomes porous, entirely similar to certain lavas of our modern or ancient volcanos, with which it is then difficult not to confound them.

Their fracture grained and earthy, and they contain quartz crystals.

Their fracture is grained and a little earthy, and small crystals of quartz may be distinguished in them by the naked eye, and almost always angular fragments of gray or milky quartz; a powerful magnifying lens causes in some fragments the discovery of needles of schorl, the amphibole of Hauy, and some small tourmalines.

Needles of schorl, amphibole, and tourmalines.

Their smell argillaceous.

Their smell is strongly argillaceous on breathing on them with the mouth.

Give no sparks.

There is no fire produced by the use of the steel, when it does not strike a quartose particle.

Do not effervesce.

They do not effervesce with acids either cold or hot.

Are affected by the magnet.

The magnet acts a little on these oxides before calcination, and strongly, or perceptibly, after it.

Weight of a cubic foot.

The medium weight of a cubic foot is 125°; that of the Italian puzzolana is but 91°.

Analysis by the moist Way.

Analysis in the moist way of the iron oxides,

I shall not weary the assembly by a detail of the manipulations relative to the solvents and re-agents which art uses for the decomposition of bodies, and shall only say, that silex, iron, alumen, and a small portion of manganese, are the constituent parts of these oxides.

I repeated

I repeated these experiments many times, and had for a medium result from an hundred pounds, chemical weight,

50	parts of silex;
31	—— of iron;
16	—— of alumen;
3	—— of manganese, and loss.
100	

If this analysis be compared with that of the puzzolana of Italy, which contains in 100 parts— and of the Italian puzzolana.

50	of silex;
25	of alumen;
16	of iron;
3	of lime;
6	of loss;
100	

their respective properties may be appreciated according to the proportions of their integrant parts.

The excess of alumen causes the plaisters made from the Italian puzzolana to crack and chap in the open air: this fault arises from their great oxidation. I have been able to replace in them those principles which they lost by decomposition. Excess of alumen causes the Italian kind to crack.

Analysis in the dry Way.

I endeavoured to obtain a regulus from these oxides of iron by using a violent heat. I followed the process of Kirwan for the fusion of siliceous and argillaceous ores of iron; yet I never obtained a single metallic button; and only found at the bottom of the crucible a vitrified mass of an opaque black, or a scoria in the state of crude cast iron. Analysis in the dry way by Mr. Kirwan's processes; afforded no metal.

Desirous to know if I could procure a malleable button by using the blowpipe, taking borax for the flux and supporting the oxide on charcoal; I still could only obtain a spongy ingot resembling crude cast iron, and breaking both when hot and when cold. The blowpipe tried unsuccessfully.

Being placed on a support of glass (according to my method published in the Journal de Physique, Tome XXXI. pages 116 and 139), the oxide fused at the second attempt, the support The oxide fused on glass.

port was coloured green, and ſmall grains of iron were ſeen to paſs firſt of a dark-green colour, then of a bright green, and afterwards to diſappear in evaporating. There remained on the globule only a ſlight tinge of blackiſh-green.

General reſult.

The reſult of all theſe facts ſeems to be, that this oxide is entirely deprived of its metallic principle, and that its ſuper-oxigenation renders it reducible and refractory.

The oxide may be uſed as a pigment.

The arts may draw ſome advantage from theſe oxides, by uſing them in pigments for buildings. I ſucceeded after many waſhings, in extracting from them a beautiful brown-red colour equal to that of commerce, and applied it to uſe ſucceſsfully.*

* This paper has been abridged in its introduction, in the details relative to negociations with the Conſtituent Aſſembly, and in ſome other points a little irrelevant to the puzzolana; but all matters directly tending to illuſtrate its nature and properties have been carefully copied.

M. Dodun's diſcovery may be of ſome uſe to this country, as there are in many parts of it large maſſes of iron-ſtone, and ſome is found in the vicinity of moſt coal-mines.

It has been long known that iron ochres have the ſame property of forming puzzolana with lime, when properly roaſted, and this circumſtance is mentioned at large in Chaptal's Chemiſtry. A patent has alſo been obtained in this country for the application of iron pyrites to the ſame purpoſe, the right to which was purchaſed long ago by Mr. Samuel Wyat. But the novelty of M. Dodun's diſcovery is, that poor iron-ſtone is equally fit for this uſe, as the other ſubſtances mentioned, which is of the more importance as it is very plentiful, and may often be procured in ſituations where the others cannot.

It may not be amiſs to mention here, that baſalt treated in the ſame manner, has the ſame property as the puzzolana: the whinſtone, of which the ovoidal paving-ſtones conſiſt moſtly, is of this kind; and it is found in great abundance in theſe countries, in different forms.—B.

VIII.

Experiments and Observations upon the Contraction of Water by Heat at low Temperatures. By THOMAS CHARLES HOPE, *M. D. F. R. S. Ed. Professor of Chemistry in the University of Edinburgh. From the Edinburgh Philosophical Transactions, for* 1804.

TO the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for some degrees of temperature above it, it seems to afford a very singular and curious exception.

Expansion of water by cold, &c. near the freezing point.

The circumstances of this remarkable anomaly have been for some time believed to be the following:

When heat is applied to water ice cold, or at a temperature not far distant, it causes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41st degree. Between this point and the 42d or 43d, it suffers scarcely any perceptible change; but when heated beyond the last-mentioned degree, it begins to expand, and increases in volume with every subsequent rise of temperature.

General or usual statement of the facts. Ice-cold water contracts by increase of temp. to about 42°, and then expands;

During the abstraction of caloric, the peculiarity in the constitution of water equally appears. Warm water, as it cools, shrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then suffers a loss of two degrees without any alteration of density. But when farther cooled, it begins to dilate, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as soon as the water reaches the 32°, or after it has descended any number of degrees below it.

and suffers the reverse change by cooling.

Supposing this peculiarity of water to be established, it must appear, indeed, a very odd circumstance, that heat should produce contraction in this fluid, while it causes expansion in other bodies *; and no less strange, that within one range of tempera-

This supposed peculiarity has

* Is this mode of change peculiar to water?—I do not know of any experiments with other fluids, except that mentioned on page 343. Perhaps it may be common to all, or at least to all those which expand by congelation. Decisive trials of this point are the more desirable, because some of Count Rumford's general inductions require or suppose that seawater should not be thus affected.—N.

 ture

ture it ſhould contract, and in another expand, the very ſame ſubſtance. Before a deviation from ſo general a law ſhould be received as matter of fact, the proofs of its exiſtence ought to be clear and indiſputable.

been hitherto deduced from experiments in narrow necked veſſels,

The experiments hitherto publiſhed, from which this ſingularity has been deduced, have all of them been performed upon water contained in inſtruments ſhaped like a thermometer glaſs, and conſiſting of a ball with a ſlender ſtem; and the expanſive or contractive effects of heat and cold have been inferred, from the aſcent or deſcent of the fluid in the ſtem.

of which the capacities alſo vary by change of temperature.

To ſuch experiments it has been objected, that the dimenſions and capacity of the inſtrument undergo ſo much change, from variation of temperature, that it is difficult, if not impoſſible, to determine how much of the apparent anomaly ought to be imputed to ſuch changes, and that it is not improbable that the whole of it may be aſcribed to them.

The author ſhows the effect by other means.

The object of this paper, which I have now the honour to read to the ſociety, is to prove by a ſet of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water preſents itſelf as that ſtrange and unaccountable anomaly which I have already deſcribed.

Previous hiſtory.

It is worth while, before detailing my experiments, to give a ſhort account of thoſe obſervations which led to the diſcovery of the fact, and which in ſucceſſion have extended our knowledge of it, as well as of thoſe obſervations which have at different periods been offered to diſcredit, and to bring it into doubt.

Dr. Croune firſt obſerved that water appears to expand before it freezes.

The firſt obſervation relative to this ſubject was made by Dr. Croune, towards the cloſe of the 17th century, while engaged in inveſtigating the phenomena of the great and forcible, though familiar, expanſion which happens to water at the inſtant of freezing; a matter which occupied in a conſiderable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that inſtitution.

His narrative. The experiment ſhewed that water roſe in a long necked veſſel by cooling:

I ſhall relate in his own words his firſt obſervation: " I filled a ſtrong bolt-head about half-way up the ſtem with water, a day or two before the great froſt went off, marking the place where the water ſtood; and placing it in the ſnow on my leads, while I went to put ſome ſalt to the ſnow, I found it above the

the mark ſo ſoon, that I thought the mark had ſlipt down, which I preſently raiſed to the water, and as ſoon as ever I mixed the ſalt with the ſnow, the water roſe very faſt, about one-half inch above it. I took up then the glaſs, and found the water all fluid ſtill: it was again ſet down in the ſalt and ſnow; but when I came about an hour after to view it, the ball was broke, and the water turned to hard ice, both in the ball and ſtem *."

From this experiment Dr. Croune drew the concluſion, that water, when ſubjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr. Hooke immediately expreſſed ſtrong doubts, and aſcribed the aſcent of the water in the neck of the veſſel to the ſhrinking of the glaſs occaſioned by the cold.

Whence he inferred an actual expanſion. But Dr. Hooke aſcribed the effect to the veſſel.

To obviate this objection, and to preclude, as far as was poſſible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immerſed in a mixture of ſalt and ſnow, and into it, when cooled, was poured, to a certain height, water previouſly brought to near the freezing point. The water began inſtantly to riſe as before, and when it had aſcended about one-fourth of an inch in the ſtem, the veſſel was taken out, the whole water remaining fluid.

Dr. C. repeated his exp. with the ſame event, in a veſſel previouſly cooled;

Theſe experiments, ſupported by others of a ſimilar nature, communicated by Dr. Slare to the Society on the 20th of the ſame month, appear to have ſatisfied its members, in general, of this fact, that water, when on the point of congealing, and while ſtill fluid, is actually ſomewhat dilated previous to the remarkable expanſion which accompanies its converſion into ice.

which gave ſatisfaction;

Dr. Hooke, however, continued unſhaken, and retained the doubts he had expreſſed.

but not to Dr. Hooke.

Remarkable as the fact, as now ſtated, muſt have appeared, it ſeems not to have excited particular attention, nor to have ſolicited more minute examination; and indeed though philoſophers did not loſe ſight of it, yet for near a century no one inveſtigated it more carefully. Mairan, in his treatiſe on ice in 1749, and Du Creſt in his diſſertation on thermo-

* Birch's *Hiſtory of the Royal Society*, Vol. iv. p. 263.

meters

meters in 1757, appear to be well aware of this property of water, but it is to M. De Luc that we owe the knowledge of the leading and more interesting circumstances, (*vide Recherches*, &c. 1772.)

Modern experiments of De Luc.

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the investigation, while engaged in ascertaining the phenomena of the expansion and contraction of different fluids by heat and cold.

He used thermometer glasses, and found the water to descend by cooling till 41°, and then rise till freezing;

He employed in his experiments thermometer glasses; and the included water, at or near the term of liquefaction, descended in the stem, and appeared to him to suffer a diminution of bulk by every increase of temperature, till it arrived at the 41st degree. From this point its volume increased with its temperature, and it ascended in the tube. This fluid, when heated and allowed to cool, seemed to him to contract in the ordinary way, till its temperature sunk to the 41°, but to expand and increase in volume, as the temperature fell to the freezing point.

The density of water, he thence inferred, is at its maximum at 41°, and decreases with equal certainty whether the temperature is elevated or depressed.

so that its density at 50° and at 32° appears the same.

M. de Luc says, indeed, that very nearly the same alteration in volume is occasioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increase or of diminution; and consequently that the density of water at temperature 50, and at temperature 32°, is the same.

His theory.

This philosopher did not conceive that the constitution of water, in relation to caloric, undergoes a change at the temperature of 41°, such that short of this degree caloric should occasion contraction, and beyond it expansion. He imagined that heat in all temperatures tends to produce two but quite opposite effects on this fluid, the one expansion, the other contraction.

In low temperatures, the contractive effects surpass the expansive, and contraction is the consequence: In temperatures beyond 41°, the expansive predominate, and the visible expansion is the excess of the expansive operation over the contractive.

In

In 1788, Sir Charles Blagden added the curious obfervations, that water, which by flow and undifturbed refrigeration permits its temperature to fall many degrees below its freezing point, perfeveres in expanding gradually as the temperature declines; and that water having fome muriate of foda or fea-falt diffolved in it, begins to expand about the fame number of degrees above its own term of congelation that the expanfion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philofophical Tranfactions*, 1801), he, or rather Mr. Gilpin by his direction endeavoured to afcertain, by the balance and weighing bottle, the amount of this change of denfity caufed by a few degrees of temperature.

Sir Cha. Blagden's obf. that water may be cooled many degrees below 32° without freezing, and continues to expand.

Every one muft be familiar with the ufe which Count Rumford has made of this peculiarity in the conftitution of water, in explaining many curious appearances that prefented themfelves in his experiments upon the conducting power of fluids, and in accountiug for certain remarkable natural occurrences. The Count, with his ufual ingenuity, has endeavoured to point out the important purpofes which this peculiarity ferves in the economy of nature, and to affign the final caufe of fo remarkable an exception from a general law.

Count Rumford's applications of this doctrine to the œconomy of nature.

In recording the obfervations and opinions that have been publifhed concerning this point, I might now, in order, notice thofe of Mr. Dalton of Manchefter. related in the fifth volume of the *Manchefter Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr. Dalton himfelf has called in queftion the accuracy of the conclufion which had been drawn from his experiments, and from thofe of preceding obfervers, I fhall only remark, that they are of the fame nature, and nearly to the fame purport, as thofe of M. de Luc.

Mr. Dalton's experiments,

It was in confequence of a communication with which Mr. Dalton favoured me, three months ago, that my attention was directed to this fubject. He informed me, that after a long train of experiments he was led to believe that he, and his predeceffors in the fame field of inveftigation, had fallen into a miftake with regard to the contraction of water by heat, and its expanfion by cold, in confequence of overlooking or underrating the effect which the change in the capacity of the thermometer-fhaped apparatus employed, muft occafion on the apparent volume of the fluid. He ftated, in general terms,

who queftions the truth of the conclufions,

because the point of greatest apparent density is different in different vessels; viz. in earthen-ware at 34°, glass 42°, brass 46°, and lead 50°.

that on subjecting water to different degrees of temperature, in instruments made of different materials, he found the point of greatest density was indicated at a different temperature in each.

In an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glass at the 42d; of brass at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the substance of the instrument, he conceived that the appearance of anomaly in this fluid originated entirely in the containing vessel, which must cause the fluid in the stem to fall or rise according as its expansions are greater or less than those of the included liquor.

A detail of these important experiments has, ere now, been transmitted for publication in the *Journals of the Royal Institution of London* *.

Mr. Dalton supports Dr. Hooke.

I have already noticed that Dr. Hooke endeavoured to explain in the same manner the original experiment of Dr. Croune. This explanation apparently gathers much force from these experiments of Mr. Dalton.

De Luc and Blagden were attentive to the vessel:

It is proper, however, to state, that M. de Luc was perfectly aware of the alteration in the dimensions of his glass apparatus, but deemed the change too trifling to have any material influence.

Sir Charles Blagden paid greater attention to the circumstance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

When it is considered, that the whole amount of the apparent change is but very small, and that the expansibility of the glass is with difficulty ascertained, and is variable by reason of the fluctuating proportions of its heterogeneous constituents, it must be acknowledged, that precision in such a calculation cannot possibly be attained, and can scarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr. Hooke, and in some measure liable to the objection which he had urged. I confess, that the experiments of Mr. Dalton, in perfect concurrence with that explanation,

and various reasons afford ground for doubt.

* They were transmitted to our Journal by the author in Vol. X. page 93.

created

created confiderable doubts refpecting the exiftence of the peculiarity of water; againft the probability of which circumftance, all analogical reafoning, and every argument *à priori*, ftrongly militate.

Unwilling to remain in uncertainty, and confidering it as a point of much curiofity and intereft, I have endeavoured to inveftigate the fubject by experiments conducted in a totally different manner, equally calculated to exhibit the fingular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimenfions of the inftrument, fhould have any influence whatever.

The author's experiments were not made uncertain by the caufes before ftated.

I have already taken occafion to ftate, that the purpofe of this paper is to prove, by experiments on the principle now mentioned, that in the conftitution of water there really exifts the fingularity often noticed.

I fhall firft ftate the plan of the experiments, and then detail the particulars of the moft remarkable of them.

When any body is dilated, whether by heat or cold, it neceffarily becomes lefs denfe, or fpecifically lighter; and the oppofite effects refult from contraction. This is the circumftance, as every one knows, which caufes various movements among the particles of fluids, when any inequality of temperature prevails in the mafs; hence thefe particles are little acquainted with a ftate of reft.

His attention was directed to fhew whether water rifes or finks by the preceding changes of temperature;

If a partial application or fubtraction of heat produce an inequality of denfity in a mafs of fluid, the lighter parts rife to the furface, or the denfer fall to the bottom.

It readily occurred, that I might avail to myfelf of thefe movements, and upon ftatical principles determine the queftion in difpute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to obferve, by means of thermometers, what fituation the warmer, and what the cooler parts of this fluid affected.

which could be done by thermometers duly placed.

If I fhould find that ice-cold water, in acquiring temperature, fhowed, in its whole progrefs, the warmer parts near the top, it would indicate that water follows the ufual law, and is expanded like other bodies by heat.

Or if I fhould obferve that warm water, in cooling to the freezing point, had the coldeft portion uniformly at the bottom, the

For the cold portions of water during

change of temperature would constantly be at the bottom, if denfeft, throughout fuch change.

the fame conclufiou would follow; while a different inference, and the exiftence of the fuppofed anomaly, would be deducible fhould the event prove different. The only circumftance, I can figure to myfelf as tending in any meafure to render this mode of examining the point doubtful, is, that water near its congealing point may have fo little change of denfity occafioned by a fmall variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent mafs, from affuming that fituation which their fpecific gravity would allot to them.

It will appear, however, very clear, from the circumftances of the experiments which I fhall immediately detail, that no obftacle to the fuccefs and precifion of the experiments proceeded from this fource.

It is not neceffary for me to relate all the experiments I have made. I fhall reftrict myfelf to the detail of fix, which prefent varieties in the modes of procedure, and which afford the moft ftriking refults.

Exp. I. Ice-cold water expofed to a warm atmofphere, was warmer ($1\frac{1}{2}°$) below till 38°, after which it was warmer at top.

Exp. I. I filled a cylindrical jar of glafs $8\frac{1}{2}$ inches deep, and $4\frac{1}{2}$ in diameter, with water of temperature 32°, and placed it on a table, interpofing a confiderable thicknefs of matter poffeffed of little power of conducting heat. I fufpended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the fame diftance below the furface. The jar was freely expofed to the air of the room, the temperature of which was from 60° to 62°.

The experiment commenced at noon:

		Top Thermom.		Bottom do.
		32°	-	32°
In 10 minutes,	-	33+	-	34+
— 30 ———	-	35.5	-	37
— 50 ———	-	37	-	38+
— an hour,	-	38	-	38+
— ——— and 10 minutes,		42	-	38.25
— ——— —. 30 ———		44	-	40
— ——— —. 50 ———		46+	-	41+
— 2 hours and 10 minutes,		48	-	42.5
— ——— —. 30 ———		50	-	44
— ——— —. 50 ———		50.5	-	45
— 4 hours,	-	54	-	49

Confiding

Confiding in the indications of the thermometers, from this experiment we learn, that when heat flows on all fides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually defcend, and take poffeffion of the bottom of the veffel.

This downward courfe proclaims an increafed denfity, and teftifies that the cold water is contracted by heat. As foon, however, as the fluid at the bottom exhibits a temperature of 38°, this courfe is retarded and foon flopped, and with the rife of temperature beyond 40° is totally changed; for when the mafs attains this degree, the experiment equally fhows, that the warmer fluid afcends and occupies the fummit, by its route announcing its diminifhed denfity, and proving that water is now expanded by heat.

Whence it is concluded that the colder (upper) fluid was rarer at the temperatures near freezing; and the warmer (upper) fluid was rarer at the higher part of the fcale.

Exp. II. I filled the fame jar with water of temperature 53°; and that I might obferve the phenomena of cooling, I placed it in the axis of a much larger cylindrical veffel, nearly full of water, of temperature 41°, and, by an earthen-ware fupport, raifed it about three inches from the bottom, taking care that the water fhould be on the fame level in both veffels. As foon as I had adjufted the two thermometers, as in the former experiment, I obferved that the top of the fluid was ftill at 53°, but the bottom had fallen to 49°.

Exp. 2. Water at 53° was every where cooled by enveloping the veffel with ice-cold water. It was warmer at top till 42°, after which it was warmer (4°+) below.

	Top.		Bottom.
In 9 minutes,	52°	-	45
— 15 ———	52	-	44

Now, to accelerate the cooling, I withdrew by a fyphon the water from the large cylinder, and fupplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes,	48°	-	42+
— 38 ———	44	-	40
— 43 ———	42	-	40
— 46 ———	40	-	40
— 52 ———	36	-	40
— 58 ———	35—	-	39
— 65 ———	34	-	37
— 75 ———	34	-	36
— 103 ———	34	-	34

This

This experiment is the counterpart of the foregoing, and from the teſtimony of the ſame inſtruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes poſſeſſion of the bottom of the veſſel, ſo as to eſtabliſh a difference of temperature from the ſurface, amounting ſometimes to 8°. And that as ſoon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that ſituation is ſtationary till the ſurface reaches the ſame point.

Whence the ſame concluſion is deduced as in the former experiment.

During the ſubſequent refrigeration, the progreſs of the cooling undergoes a total change. The thermometers tell that the colder fluid riſes to the ſurface; ſo that the top gets the ſtart of the bottom ſoon by 1°, and attains the loweſt temperature of 34° very long before the other falls to the ſame degree.

Theſe circumſtances, I think, lead to the concluſion, that by the loſs of caloric, water at 53° is contracted and rendered ſpecifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an oppoſite effect is produced; for now the water, as it cools, becomes ſpecifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the ſituation, which the warmer and colder parts of the fluid affected, in the progreſs both of the heating and cooling, while every external circumſtance of the proceſs continued unaltered, is particularly worthy of remark.

Exp. 3. A tall jar, nearly 18 inches high, containing water at 50°, was cooled round its *upper part* by ice and ſalt. The temperature fell quickeſt at bottom, till 40°, where it continued ſtationary; after which the ſurface ſunk to freezing, and congealed.

Exp. III. I took a glaſs jar, 17.8 inches deep, and 4.5 in diameter internal meaſure, having a neck and tubulature very near the bottom. I provided alſo a cylindrical baſon of tinned iron, 4.8 inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I ſecured this baſon, ſo that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the ſuperior portion of a cylinder of water, and it anſwered the purpoſe completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the riſing of the bottom, and having fixed it in this ſituation, I rendered the aperture water-tight, by a perforated cork and lute.

This

This very tall jar was placed on a table, with the interpofition of fome folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the bafon, which embraced the top, a mixture of powdered ice and falt.

From time to time I explored the temperature near the furface, by inferting the bulb of a thermometer to the depth of half an inch nearly in the axis.

The experiment lafted 50 hours.

	Bottom.	Top.	Air.
One o'clock,	50°	50°	42°
In 11 minutes,	46 +	—	
— 15 ———	45	48	
— 21 ———	44—	46—	
		44	
		42	
— 51		34	At this time a thin film of ice began to form in contact with the glafs.
— 1 hour [illegible] min.	40	34	
— ——— 20 ———			
— ——— 44 ———			
— $4\frac{1}{2}$ hours,			A cruft of ice of fome thicknefs now lined the glafs, and air had fallen to 40°.
— $5\frac{1}{2}$ hours,	39		
— 11 hours, *i. e.* at midnight,			Cruft of ice complete.
— 19 hours, *i. e.* next morning,			Air 40°.
— 26 hours,	40		Air 40°. So much ice had melted that the cake was detached from the fide of the veffel, and floated.
— 32	40		
— 41	40		Air 41°. Ice not all melted.
— 50	41		Air 42°. Ice not entirely gone.

Review of the facts and remarks.

This long protracted experiment prefents fome ftriking facts, and its general import, with regard to the fubject of inveftigation, agrees with the preceding. In it we fee, that when the frigorific mixture abftracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared fooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a

 half

half inches distant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condensed fluid descending, and a corresponding one of a warmer temperature ascending. Now, if water observed the same law that other bodies do, and had no peculiarity of constitution, the same progress of cooling should continue. This, however, the experiment teaches us, is not the case: as soon as the fluid at the bottom exhibits a temperature of 40°, it ceases. The colder fluid remains at top, and quickly losing temperature, ere long begins to freeze. The continuance of the colder fluid at the surface surely denotes, that it is not more dense than the subjacent warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

As the fluid below 40° continued at top, it was not *denser* than that at 40°.

Did water observe the usual law, and lose volume along with temperature, this experiment, by its long duration, afforded ample time for the manifestation of it.

For not less than two days did ice-cold water maintain possession of the top, and for the same period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condensed fluid moved from the surface, to affect the inferior thermometer, or to attest the contraction of water by cold.

Yet the experiment does not show that it was *rarer*.

This experiment, however, I must remark, does not warrant the conclusion, that the water is actually expanded, though it in no degree opposes it. It proves no more, than that the contraction ceases at 40°; and that water of 32° is not more dense than of 39° or 40°. Nay, some may perchance alledge, that it does not prove so much; conceiving, that if at 40° the contraction, without ceasing altogether, becomes very inconsiderable, the difference of density occasioned by the subsequent reduction of temperature may be so very trifling, as not to enable the cold particles to take that situation which their gravity assigns to them, in opposition to the inertia and tenacity of the subjacent mass; and therefore that the colder, though heavier fluid, may be constrained to remain above. That this allegation should have no weight attached to it, the circumstances of the succeeding experiment will clearly show, as I shall soon notice.

It might be even alledged that a small excess of density prevailed;

but this is not entitled to regard.

That heat which passed by direct communication

Before quitting the consideration of the present experiment, it may be worth while to remark, that it may seem rather surprising,

prising, that the bottom of the fluid was not apparently affected in its temperature by the ice which so long occupied its surface. It might be expected, though no cold currents descended from above, that the caloric should be conducted from below, and that the temperature should by that have been reduced,*. I suppose that the caloric did pass from the lower strata upwards, but extremely slow, by reason that fluids, as Count Rumford taught us, are excessively bad conductors of heat, and so very slowly, that the caloric entered from the atmosphere with sufficient quickness to prevent any depression of temperature below the 39th degree.

must have been very slowly transmitted.

This experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to ascend."

* ANNOTATION, BY THE AUTHOR, †

This experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count Rumford, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid state, seeing that for two days the fluid at the bottom of the vessel never fell below 39°, though the surface was at 32°.

The opinion of Count Rumford that fluids cannot conduct heat from particle to particle,

From the circumstances detailed in his seventh essay, the Count concluded, that heat cannot descend in a fluid. From the present, it might with equal justice be inferred, that heat cannot ascend.

Had I not the fullest conviction that this celebrated philosopher has pushed his ideas too far, I might be disposed to consider this experiment as according well with the hypothesis.

appears to be inaccurate.

Soon after the interesting speculations of the Count appeared, I began to investigate the subject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, satisfied myself that he assigned to fluidity a character that does not belong to it. Though since the date of these experiments, the public has

† As this note subjoined at the foot of the page after the words *temperature should by that have been reduced*, in the original, is of such considerable length, I have taken the liberty of putting it in the same type as the text.—N.

become

become possessed of several series, well devised, and, in my opinion, of themselves conclusive, it may yet be worth-while to state the tenor and result of them, by which the value of their testimony in favour of the conducting power of liquids may be estimated.

Experiments to shew that heat can descend in fluids:

The experiments were of two descriptions.

The one set, of the same nature nearly with those of Count Rumford, was designed to examine, Whether heat, when applied to the surface, can descend in a fluid; and the other to discover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the subjects of the Count's experiments, were employed for the first set.

To water (and to oil) the heat was communicated from the bottom of a metallic vessel (in contact with the surface of the fluid, and) heated by boiling water within.

To explore the conducting power of water and oil, the apparatus which I used consisted of two vessels of tinned iron, both cylindrical, and the one somewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the subjects of the trial, to different depths on different occasions. The smaller was ten and a half inches in diameter. By three hooks it was suspended within the larger pan, in such a manner, that the bottom of it exactly reached and came in contact with the surface of the fluid. This smaller vessel became the source of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the use of a syphon, all agitation and disturbance.

This hot vessel did not touch that which contained the fluid under experiment; and the containing vessel was kept cold round the surface of the fluid, and therefore did not carry any heat downwards.

In experiments of this nature, the difficulty is to prevent the conveyance of caloric by the sides of the vessel. I attempted, and, I think, I succeeded, in overcoming this difficulty, by encircling the larger vessel, as a height exactly corresponding with that of the surface of the fluid within, with a gutter or channel about half an inch in depth; and by causing a stream of cold water to flow constantly through a syphon into this gutter, while from the opposite side it ran off by a small spout.

The water was several degrees colder than the subject of the experiment, and keeping cool the portion of the vessel with which it was in contact, it intercepted the heat that would otherwise have travelled by this route to the bottom.

Mercury was tried in glass vessels.

For mercury I had recourse to vessels of glass.

In

In all the experiments a thermometer bore testimony that the caloric descended from the surface to the bottom of the fluid, and demonstrated, at least to my conviction, that fluids can conduct heat.

In all the experiments the heat descended

The progress of the heat, however, was very slow, and attested the important fact, for which we ought to be thankful to the Count—That fluids are very bad conductors.

slowly.

The second set of experiments was calculated to examine, in a very different manner, the position, That all interchange and communication of heat between the particles of fluids is impossible.

When a hot and a cold fluid are mixed together and well agitated, very soon an uniform is produced. This equality must proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count Rumford. To which cause it ought to be attributed, I conceived I might discover, by ascertaining whether, after such an intermixture, any separation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as soon as the agitation ceases, ought, by reason of their greater rarity, to accumulate, to a certain degree, at the surface, and there exhibit a temperature above the common one.

Other experiments of mixing fluids.

I first tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a stoppered glass jar, I shook them well for a short time.

I then noticed the resulting temperature, and raising the ball of the thermometer towards the surface, I had an opportunity of observing, that it never rose the smallest portion of a degree above the common temperature which had been established.

When hot and cold water are mixed, they acquire a common temperature throughout, and never separate.

I next made a similar experiment with alcohol, selecting it on account of its remarkable dilatability. I shook well, for half a minute, a mixture of equal parts of alcohol at tempe. 40° and at temperature 170°. The resulting temperature of the mass was 140°.

The same effect was found with alcohol.

Now, if this was a mixture of particles at 40° and at 170°, as the difference of specific gravity between the fluid at these temperatures is very considerable, some separation of the warmer

and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never ascended above the point of equilibrium.

From these experiments I concluded, that the uniformity of temperature was established by an actual communication and interchange of heat between the particles.

Count Rumford remarked that the mixture might be too complete to allow of separation:

It may not, however, be improper to state, that Count Rumford, with whom several years ago I had the pleasure of conversing upon this subject, alledged, that the intermixture might be so complete as to prevent any separation whatever.

but oil and water do acquire the common temperature by mixture, and exhibit the same when separate.

If it be a property essential to fluidity, that heat cannot pass from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the speculation, the the caloric is so communicated Though, *à priori*, I entertained no doubts respecting the result of the experiment, I poured a quantity of olive oil which had been heated by immersion in a vessel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by shaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the the water; for when the fluids separated, and had arranged themselves according to their specific gravity, both of them had the same temperature of 78° *.

The experiments of the two descriptions now recorded, left on my mind little doubt that the Count had overstrained his conclusions.

Exp. 4. The tall jar of Exp. 3, containing water at 40°, was cooled round its *lower part* by ice and salt.

† *Exp.* IV. I took the same tall jar, and stopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan After suspending two thermometers, as in experiment first and second, I poured a mixture of ice and salt into the pan, to the depth of 4.2 inches, the air of the room being 40°, as in the last experiment.

The temperature fell as quickly at top as at bottom:

	Bottom.	Top.	Air.
Eleven o'clock,	40°	40°	40°
In 10 minutes,	38+	38+.	

* This is also very strikingly the case with mercury and water.—N.

† The text is here resumed.

In

	Bottom.	Top.
In 20 minutes,	38°—	38°—
— 30 ———	37—	37—
— 40 ———	36	36
— 60 ———	35.5	35.5
— 80 ———	35	35
- 100 ———	34.5	35
- 120 ———	34—	34
— 8 hours.	34—	34

A cruſt of ice began to form on the inſide of the glaſs when the water in the axis of the bottom and of the top was at 36°. In the courſe of the experiment, it became at leaſt an inch thick.

We learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as ſpeedily perceived at the ſummit as in the axis of that part, on the external ſurface of which it immediately acts. As fluids conduct heat ſo very tardily, this can only ariſe from currents of cooled water aſcending from the bottom, and theſe cold currents cannot move upwards, were not the water of them ſpecifically lighter than that of the incumbent warmer fluid.

whence it is concluded that the cooled water aſcended from the bottom from its greater rarity below 40°;

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, muſt be expanded by the loſs of caloric.

This experiment ſecures full force to the laſt, as it obviates the objection already noticed, and alſo precludes another. I have already ſtated, that it may perhaps be alledged, that the fluid at the top, in experiment third, though cooled to 32° did not deſcend, becauſe below 40°, the contraction is ſo trifling, that it does not occaſion a difference of ſpecific gravity ſufficiently great to cauſe the particles to deſcend, when oppoſed by the inertia and tenacity of the fluid through which they have to fall: or it may be conceived, that the deſcent is ſo tardy, that time is given to the ambient air or ſubjacent fluid to furniſh heat enough to raiſe the temperature of the deſcending ſtream, and by that arreſt it in its downward courſe.

and the objections to Exp. 3. are removed.

But from the particulars above recorded, it is manifeſt, that the change of denſity between the temperature of 32° and 40° is quite ſufficient to put into motion the particles, and to enable

enable them to overcome the obſtacle ariſing from inertia and tenacity, and to withſtand the arreſting effects of atmoſpheric heat.

Though theſe experiments, and ſome others of a ſimilar nature, carried conviction to my mind, and perfectly ſatisfied me reſpecting the reality of the anomaly of water, I determined to vary ſomewhat the mode of making the experiment, ſo as to obtain ſtill more ſtriking reſults.

Another experiment with a ſtill taller jar, 21 inches high.

For the fifth experiment, I uſed an apparatus which conſiſted of a ſtill taller jar. It was 21 inches high, and 4 in diameter. I adjuſted at the middle of its height a perforated baſon of tinned iron, 2 inches in depth, and 10 in diameter. As this baſon embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what courſe the heated or cooled fluid ſhould take.

Exp. 5. The laſt mentioned jar was filled with ice-cold water. Heat was applied to a zone of two inches near the middle by means of warm water in a circumambient veſſel.

Exp. V. I filled the jar with water at 32°. I placed it upon ſeveral folds of thick carpet, previouſly cooled to the ſame degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments firſt and ſecond. I then poured water of temperature 68° into the baſon, and by means of a ſpout ariſing from the ſide of it, and a ſyphon connected with a reſervoir of water at the temperature now mentioned, I renewed the contents of the baſon frequently, but without cauſing any agitation.

The temperature roſe below to 36° but remained unchanged at top: but after the lower water had attained 39° it became ſtationary, and the temperature at top ſoon roſe to 48.°

	Bottom.	Top.	Air.
At commencement,	32°	32°	33—35
In 10 minutes,	35	32	
— 15 ———	36—	32	
— 20 ———	36+	32	
— 25 ———	37	33	
— 30 ———	38	33	*** From this time I charged the baſon with water of temperature 88°, and renewed it frequently.
— 38 ———	38+	33	
— 45 ———	39—	33	
— 50 ———	39+	44	
— 55 ———	39+	45	
— 60 ———	39+	48	

Nothing can be more deciſive with regard to the queſtion in diſpute, than the particulars of this experiment. Heat is applied

applied to the middle of a column of ice-cold water. The heated portion has an equal ſhare of the column of cold fluid above it and beneath it. There is nothing to determine its courſe in one direction or another, excepting its actual change of denſity.

The thermometer evinces that the warm current ſets downwards, and carries the increaſed temperature to the bottom. There, this inſtrument indicates the ſucceſſive riſe of ſeveral degrees, before the ſurface indicates the ſmalleſt acquiſition of heat.

Whence it is deduced, that a *warm* current of water between 32° and 39°+ deſcended becauſe *denſer* and that when the temperature was more than 39+ the *warm* current aſcended becauſe *rarer*.

The inference is plain, that the cold water is contracted by the heat.

The change of the effect of heat is equally well illuſtrated by this experiment.

No ſooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its courſe, and, by aſcending, carried the increaſe of temperature very rapidly to the ſurface, ſo that it ſoon ſurpaſſed the bottom, and continued to riſe, while the other remained ſtationary.

Exp. VI. I filled the jar uſed in the laſt experiment with water of temperature 39½°, the air and the ſupport being at 39°. Diſpoſing the thermometers in the uſual manner, I introduced a mixture of ſnow and ſalt into the baſon.

Exp. 6. The ſame jar was filled with water at 39½° and a freezing mixture applied to the middle zone. The fluid at bottom was ſcarcely changed, but that at top froze.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	39°
In 10 minutes, -	39+	38+	
— 25 ———	39+	36.5	* * At this time
— 35 ———	39	36−	ice began to be
— 55 ———	39	35	formed on the
— an hour and 10 min,	39−	34+	ſide of the veſ-
——— 35 ———	39−	34−	ſel.
— 2 hours, -	39−	33+	

This experiment ſpeaks in as decided language as the preceding. It ſhows that when a portion, in the middle of a column of water at temperature 39.5 is cooled, the colder fluid riſes, and does not deſcend through the warmer maſs, and preſents the unequivocal demonſtration, that water of temperature 39½° is actually expanded by loſing heat.

So that the water cooled below 39½ did riſe by expanſion.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give relative

The general fact is, that heat causes ice-cold water to contract to 40°, and afterwards to expand.

relative to the subject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes specifically heavier,—that water of 40° when heated becomes specifically lighter,—that water above 40°, by the loss of heat, or by cold, is rendered specifically heavier; and that water below 40° is, by the same cause, rendered specifically lighter.

Such being the general import, the conclusion is irresistible, that heat, in low temperatures, causes water to contract, and at superior temperatures to expand. The opinion, therefore, is founded in truth, that water possesses a peculiarity of constitution in relation to the effects of caloric, and that it is, within a short range of temperature, an exception to the general law of "expansion by heat."

The greatest density lies between $39\frac{1}{2}$° and 40°.

So far as I can judge from these experiments, I am disposed to believe that the point at which the change in the constitution of this fluid in relation to heat takes place, lies between $39\frac{1}{2}$°, and the 40th degree.

I am not at present aware of any objection to the method I have followed in establishing this singular anomaly, and in removing any doubts which may have arisen from the unavoidable influence which the instrument must have in the mode of conducting the investigation that had previously been adopted.

These experiments shew the nature of the change, but not its amount.

The plan of operation above described, however, only ascertains the fact; it gives no data for ascertaining the amount of the anomalous effect of heat.

Whether the expansions and contractions be the same at equal intervals from 40°?

I have already stated, that M. de Luc alledged, that from the temperature of 41°, the expansion occasioned by cold was very nearly equal to that produced by the same number of degrees of heat; and consequently that water possesses the same density at any given number of degrees of temperature above and below 41°. The first experiments of Mr. Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itself to be cooled before it begins to freeze. From one circumstance that constantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that caused by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was less than what occurred during the cooling or heating

ing of the fluid through the same number of degrees above it; and I conceive that, when other circumstances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a measure of the difference of density.

Alcohol, when heated or cooled, presents, by reason of its greater expansibility, a greater difference of temperature in these situations than water; and upon the same principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depression.

As the concurrence of the testimony of the experiments above related with the general opinion, will probably remove every doubt respecting the matter of fact, it remains a very difficult problem for those who are fond of philosophical investigation, to explain how heat shall occasion in the same fluid, without producing any alteration of mechanical form of chemical condition, at one time contraction and at another expansion, and to reconcile the contractive effect to the conceived notions of the mechanism of the operations of this energetic agent.

It is a difficult problem to explain, how these contrary changes by heat are effected.

When heat causes expansion, it is imagined to act by inducing a repulsion among the particles of bodies, which, opposing and overpowering the cohesive attraction, causes the particles to recede.

The question stated.

In what manner, then, the addition of heat can occasion, or allow, the particles of water to approach each other, and how the subtraction of it can make them retire to a greater distance, I confess I can in no measure comprehend.

An explanation, abundantly plausible at first view, very readily suggests itself to every one who is aware of the great and forcible expansion which happens to this fluid at the moment of its congelation. It it stated by Sir Charles Blagden, in the paper already quoted.

Sir Charles Blagden's explanation; viz.

The remarkable dilatation which water experiences at the instant of being converted into ice, is very generally ascribed, and I presume very properly, to a new arrangement which the particles assume, determined probably by their polarity; by which one side of the particle A is attractive of one side of B, while it is repulsive of another.

As water expands in freezing by virtue of a new arrangement of the particles,

Now,

it is probable, that the arrangement and the expanſion may begin before ſolidity enſues,

Now, if this polarity operates with ſo much energy as to impart almoſt irreſiſtible expanſive force at temperature 32°, it is reaſonable to ſuppoſe that it may begin to exert its influence, though in a far inferior degree, at temperatures ſomewhat more elevated. The expanſion, therefore, that takes place, during the fall of temperature from 40°, may be imputed to the particles beginning or affecting to aſſume that new arrangement which their polarity aſſigns them, in which arrangement theſe particles occupy more ſpace than before.

and the contrary.

Again, when heat cauſes water of 32° to contract, upon the ſame principle, it may be conceived to operate, by counteracting the ſmall portion of the diſpoſition to polarity that ſurvives the liquefaction.

I am afraid that we cannot reſt ſatisfied with this explanation. We muſt not be deceived by the plauſibility of it.

The ſtate of perfect fluidity depends upon the circumſtance, that the particles of any body admit of ready motion upon each other, and that the change of relative ſituation meets with little or no ſenſible reſiſtance.

Objection. This advance towards congelation, ought to impair the fluidity:

Water certainly poſſeſſes fluidity in a great degree, and its particles muſt of courſe encounter but little reſiſtance, as they glide the one upon the other. But if theſe particles ſhall begin to exert any degree of polarity, by which certain faces become more diſpoſed to attach to each other than certain others, this tendency would neceſſarily oppoſe that indifference with regard to poſition, which is eſſential to fluidity, and of courſe muſt impair the fluidity, and induce ſome degree of tenacity or viſcidity.

which does not appear to be the caſe.

To appearance, however, water at 32° has its fluidity as perfect as at temperatures conſiderably elevated. Unwilling to truſt to appearance, where experiment might decide, I have attempted in various ways to aſcertain whether the water ſuffers any ſenſible diminution in this reſpect while it is expanded by cold. The following method I deem the moſt correct.

Experiments with Nicholſon's gravimeter.

For the purpoſe, I employed a gravimeter, the one contrived by Mr. Nicholſon for diſcovering the weight and ſpecific gravity of ſolids.

This is a convenient inſtrument, but, unfortunately, it is by no means ſo ticklish as a balance. Duly loaded, ſo as to be equiponderant with the water in which it is plunged, Mr. Nicholſon

Nicholson says, it is sensible to the 20th part of a grain*. The one I have, though its stem be slender, is scarcely sensible to less than two or three twentieths of a grain.

The want of sensibility in the gravimeter arises, in a great measure, though not entirely, from a certain degree of tenacity subsisting among the particles of the fluid; and any thing that tends to increase this tenacity, must, in the same proportion, augment this want of sensibility.

To ascertain whether any sensible change in the tenacity or fluidity accompanies the expansion of water by cold, which the theory requires, I examined the mobility of the instrument when immersed in water at different temperatures. I first plunged it into this fluid, heated to between 60° and 70°. Under due loading, which sunk it to the mark on the stem, it was not sensible to a weight less than two or three twentieths of a grain.

It rose and fell in warm, and

I then tried it in ice-cold water, and found that its sensibility was in no perceptible degree impaired. The coldness of the water, it must be remembered, causes some degree of contraction of the gravimeter. This contraction cannot fail to render the instrument in some small measure more sensible, and, so far as it goes, to counteract the sluggishness produced by any increased tenacity in the fluid.

in ice-cold water with equal facility.

But as the body of the instrument is made of glass, the amount of the contraction must be very small, and the change of sensibility arising from it so very trifling, as certainly by no means to obscure such an effect as an increase of tenacity would occasion. I therefore with some confidence conclude, that the fluidity of the water is not sensibly diminished, and consequently that the polarity has not begun to exert any sensible influence; it can scarcely, therefore, be accounted the cause of the dilatation.

Whence the author concludes that the fluidity is not sensibly changed.

* Perhaps the difference of sensibility in my instrument, and that of the learned Professor, may have arisen from a difference of the diameters of the stems. Mine was of one fortieth of an inch. It was well rubbed with a clean linen cloth, which rendered the surface equally disposed either to descend or ascend; and the instrument was not judged to be in equilibrio with the fluid, except when the surface about the stem was neither prominent nor depressed. This was easily known by the reflected image of the window frame, or other objects being seen close to the stem without distortion.—N.

Annotation.—W. N.

Sir Charles Blagden's theory of expansion of water by heat does not suppose that it should be less fluid.

It does not seem to me that Sir Charles Blagden's explanation does necessarily imply that the fluidity of the mass taken as a whole, should be sensibly impaired when tried by the application of a mechanical test. It might be impaired in the same manner as the water is affected by mixing small floating fragments of a solid along with it. When a saline solution which would become solid by cold, such for example as the sulphate of soda, is cooled below its point of congelation, the crystals will be differently formed according to circumstances.

Instance of crystallization differing according to circumstances.

If the fluid be gently shaken or made to oscillate, a shower of minute crystals will gradually fall through the fluid; and the whole mass will be a considerable time before the crystallization is finished; but if, instead of this method of agitation, the glass be scratched by a quill underneath the fluid, in Sir Charles Blagden's way, or if a small instrument, having a crystal of the salt adhering to it, be dipped into the solution, the crystals will radiate with great rapidity from that centre of perturbation, and in a few seconds the whole of the solution will become rigid.

Whence it is conjectured that the expansion of water by cold may arise from minute crystals of ice in the fluid.

This common and very striking experiment of chemical lecturers, seems to me to indicate at least a possibility that small crystals of ice may be formed and float distinctly from each other in water, at 40 degrees and lower: and I think the metals afford us a number of instances in which a considerable interval of temperature is found to be between the commencement of crystallization and the solidification of the whole mass. In pewterers solder the interval is not less than 40 degrees. This hypothesis of such disseminated particles of ice, which seems to be nothing more than an expression of Sir Charles Blagden's theory in different words, will explain why the colder water should be lighter;—namely, because it must contain more ice, and also why the expansion ought not to begin but at some definite temperature.

A measure of the greater or less fluidity of bodies is very desirable.

Though it does not appear to me that the theory of Sir Charles must necessarily imply a change in the mechanical resistance of water from what may be called rigidity; yet there are many other reasons why philosophers should be desirous of measuring the variations of fluidity in bodies: that is to say, the greater or less facility with which their parts are moved amongst each other.

Dr. Hope's trial may be modified probably by a

The ingenious attempt of Dr. Hope to ascertain this from the resistance made by a fluid to the perforation

ration of its surface by a cylindrical solid, is liable to the objection that it supposes the attraction or repulsion between the solid and the fluid to remain unchanged by variations of temperature; whereas the contrary seems most probable. The doctor's experiment must be grounded upon a position that the greater the depression or the greater the elevation of a fluid round a small cylinder partly immersed in it, the greater must be the resistance from imperfect fluidity. But these effects are evidently as much governed by the attraction or repulsion of the solid with regard to the fluid as by the resistance which the experiments are intended to measure. I have somewhere read that water clocks and other instruments for measuring time, by the passage of water through small holes, go slower in cold weather. This may arise from contraction of the hole, though my author ascribes it to imperfect fluidity. After some meditation on this problem it still appears to me to be surrounded with difficulties. Perhaps it may be one of the best methods to suffer the fluid to drop from a capillary syphon in different temperatures. I am disposed to think that the drops would be smallest and the whole quantity in a given time greatest when the fluidity was the most perfect, or at least when the adhesion of the particles of the fluid to each other was the least. But even here the attraction of the small capillary extremity of the tube from which the drop would fall would require to be considered; and on this account the method would be preferable (if so) to Dr. Hope's only because the repetition of a great number of drops or quantity of effluent water would give a greater degree of precision to the result.

change in the attraction or repulsion between the water and the stem of the gravimeter.

Water clocks are said to go slower in cold weather; because the water is less fluid.

Supposition that most water would drop from a capillary tube when it was most fluid.

Is it likely that the rope pump turned regularly a certain number of turns in a given time would raise more water when coldest and least fluid? If it did not might we not infer that the fluidity of water is not sensibly affected by change of temperature?

Will the rope pump shew a difference in the tenacity or fluidity of water hot or cold?

Observations

IX.

Observations on Turf, from the German Rathbeger fur alle Stænd. *By* Doctor Collenbusch.

It is not very probable that a man placed beside a fountain of pure water should suffer himself to die of thirst through neglect of using it, or possessing food in abundance, should not appease his hunger with it; nevertheless instances of this kind are not wanting.

Wood fuel very scarce in Germany. Other matter may be substituted for it.

Every one complains in Germany of the scarcity of wood for fuel. It is known that substances have been found in other places which can supply its place, and that they have been formerly used here; but all this cannot induce any one to search for *turf*.

Ungrounded prejudices prevent the use of turf for fuel.

It is easily conceived that proprietors of woods, through the fear of having their profits diminished, should endeavour to perpetuate ancient prejudices, and to extend the opinion that the plague only ceased its ravages since the use of *turf* for fuel has been discontinued; but it is difficult to imagine that magistrates instead of encouraging the preparation of this fuel, should endeavour to prevent those from doing so, who wished to engage in it.

Used in Germany from the most remote periods.

It is very likely that the discovery of the use of *turf* as a combustible was first due to chance; and besides the use of this fuel in Germany has been continued from periods more remote than any written documents extend to.

Various erroneous opinions formed of its production and use.

The principal causes which have prevented the search after *turf*, are the erroneous opinions which have been formed of the manner in which it has been produced, of its preparation, and its use; some of which are as follow.

1st That turf is found in veins like metals.

Some think, for example, that *turf* has been formed at the moment of the creation, such as it is now found in the earth, and that there are veins of turf, as there are of iron, copper, tin, and other metals; but experience proves the falsity of this opinion, for there is found in almost all parts of Germany turf covered with more or less earth, (if only a proper search be made for it) beneath which layers of trees may be seen, which proves that there formerly were forests in the same places.

Others

Others believe that at the time of the deluge vaſt foreſts were overthrown, and afterwards covered with herbs, reeds, and other plants, and that theſe vegetables having rotted by degrees, became at laſt this black combuſtible maſs reſembling earth, which muſt have required an enormous quantity of vegetables, as plains of many leagues ſquare are found covered with beds of it to the depth of more than 25 feet, beneath which trees are diſcovered of great hardneſs, and almoſt petrified.

2d That it was formed at the deluge.

Others imagine that it is more probable that the ſea tranſported the materials of the turf from the weſtern countries to the eaſtern, and covered with them the trees which are found buried beneath the turf. It is very true, that theſe trees have their roots turned towards the weſt, and their heads to the eaſt. But then it is difficult to explain how this ſubſtance could be carried to countries diſtant from the ſea, and even to the tops of the higheſt mountains in upper Saxony, on the *Brocken* and the *Alps*.

3d That it was tranſported by the ſea from the weſt.

Many perſons are of opinion that torrents and rivers have drawn together and depoſited leaves and branches of trees on the low grounds, and that they have thus accumulated the conſtituent elements of the turf; but this cannot take place in countries in which no large rivers are found, nor on high mountains. The microſcope clearly ſhews that turf, eſpecially that kind which is from the ſurface of the earth, is compoſed of moſſes, herbs, ruſhes, and other vegetables, and their roots ſtrongly interlaced, of which the greateſt part is changed into earth.

4th That it was waſhed down by torrents.

The microſcope ſhews it compoſed of vegetable fibres.

Paper has actually been compoſed from turf, and the water which has ſettled in turbaries is uſed to tan leather, which proves that it is principally compoſed of vegetables. Chemical reſearches have alſo diſcovered in it a mineral reſin which principally promotes its combuſtibility. It appertains then partly to the vegetable, and partly to the mineral kingdom.

Paper made from it and leather tanned by its water.

Turf may be produced artificially, by digging trenches 6 feet deep, and from 15 to 20 feet ſquare; the trenches become filled with water, and produce the firſt year a green ſlimy moſs, the ſecond year this moſſy vegetation covers the water to the height of two feet, and a great quantity of filaments are diſcovered in it mixed with leaves and flowers, in the third year a ſtratum is eſtabliſhed, which attracts the duſt and the ſeeds which

Turf produced artificially by ſinking deep and wide trenches, which fill up by vegetation.

which float in the air, and engender a quantity of marsh plants, of reeds, and of herbs, which the fourth year become so heavy that they fall to the bottom. They then become compressed there, and by successive repetitions of this operation, all the trench becomes filled up in the course of 30 years; however this turf would probably require 100 years before it would equal the ancient turf.

Three species of it.

Although this turf is always the same in its constituent parts, it nevertheless differs in having these parts variously mixed, which occasions its being divided into three species. The first comprehends the *surface turf*, and is the most common kind; it is found almost every where; but it contains in some places more combustible matter, which makes its colour vary.

1st The surface turf.

Found wherever water stagnates and is covered with weeds,

This species is always sure to be found wherever places are discovered where the water stagnates, whether on plains, elevations, or declivities, in such a manner as to form a thick blueish crust, and deposits a yellow mud; or where the soil is covered with moss, reeds, rushes, or ridges; and if at the same time the feet of the passenger sinks into the loose soil, if the earth bends beneath his feet; if trees are perceived (which are commonly little pines or fir trees, or sometimes other kinds of trees,) covered with much moss, inclined to one side, and half rooted up at the other by the wind; in all these places turf will be found near the surface, and it is only necessary to remove the sod to perceive it. But this operation may be performed more quickly and easily with the English borer, which also will shew the depth of the bed.

and where trees covered with moss are half up-rooted by the wind.

To procure it the water should be drained off.

To procure the turf, the water should be drained off, which is easy to do if the country is elevated or has valleys in its vicinity; but the operation is more difficult when the earth is level. As persons are not always to be found capable of taking the levels of ground, the places should be remarked where the water settled in spring when the snow melts; these places should be marked by stakes, and afterwards the trenches should be made to pass this way which are to be dug, to let the water run off.

Easy method of finding the descent for the drains.

Method of preparing the surface turf.

To cut the turf an iron spade is used, which should be neither round nor pointed, but terminating in a straight line; this should be screwed down as far forwards as possible, along the side of a stretched cord, by a line 14 inches long and six broad; the detached part is separated from the depth of three inches

inches, at two ſtrokes of the ſpade, to the length of 16 inches, and $4\frac{1}{2}$ inches broad, and this piece of turf is afterward divided in two.

In order that the pieces of turf may dry quickly, they ſhould be placed on planks, and diſpoſed ſo that the air might freely circulate between them, and that they could receive the rays of the ſun. Method of drying it.

When the turf is thus dried to a certain degree, it is placed under ſheds to compleat the drying; for if it was expoſed to the ſun till it was entirely dry, it would loſe its ſtrength and burn like ſtraw. Should not be dried too much in the ſun,

It is alſo diſadvantageous to cut a large proviſion of it for many years, for the laſt made is always the beſt. The upper and lower beds a … alſo obſerved to be of inferior quality to thoſe in the midſt; the beſt turf is that of a brown colour inclining to black, is heavy, and its texture is traverſed by a ſmall quantity of roots; this kind produces a ſtrong and laſting fire, and its ſmell is very ſupportable. The more it is of a bright brown colour, the greater number of roots in it, and the lighter it is, the worſe is its quality. This ſort conſumes more ſpeedily, and may ſerve to advantage where a quick fire is wanted; its odour, it is true, is very diſagreeable, but its aſhes are excellent. or kept too long. Upper and lower beds of it the worſt. Beſt kind dark brown and heavy. Bright brown or reddiſh worſe.

The turf which inclines to a grey or yellow colour, and which is mixed with reed, is always the worſt ſort, but always good enough to heat kilns or ovens, and its aſhes are good; this ſpecies is ſeldom found below the depth of two ells; it is reproduced after ſeveral years. Grey or yellow ſort the worſt.

The ſecond ſpecies of turf is the *crumbling turf (moder-torf)*, this kind is found more abundantly in Holland; its cutting and preparation require much more pains than the ſurface turf. Second ſpecies, the crumbling turf.

The third ſpecies, or the *mountain turf*, is dug up from pits and galleries, and is reduced to regular forms like the preceding kind. Third ſpecies, the mountain turf.

It is objected to the uſe of turf that it cannot be employed as a ſubſtitute for wood in all the places where wood is burned; but it ſhould not be forgotten that wood itſelf is not fit for every work where fire is required; that in order to be employed in founderies it muſt be charred with much trouble, and with a loſs of two thirds of its weight, and that wood as well as turf is of different qualities and produces different effects. Wood not proper for fuel in all caſes more than turf.

Turf

Turf may be charred.

Turf is also susceptible of amelioration, especially the surface turf, the crumbling turf, and the mountain turf likewise; for it may be reduced to charcoal, and will thus serve for every work which requires fire; and in this case it yields neither smell nor smoke. The more strongly the turf is compressed before its carbonization, the more excellent is the charcoal.

Advantages from the use of turf.

To compensate for the inferiority of turf to wood, granting that it is inferior, its use will prevent the great price which will otherwise necessarily be paid hereafter for timber for building, and will admit of the woodlands being proportionally reduced; the places also where the surface turf has been dug up, if it has not been from too great a depth, may serve for situations wherein to plant cabbages, beets, and madder, or they will serve for fish ponds. The use of turf will admit of the multiplication of manufactories which use fire, of mines and forges; aged persons and children may be employed in preparing it; its ashes form a good manure, and the mould which falls from it may easily be converted into ashes.

Cabbages, &c. may be planted where it has been extracted.

Its use admits of having more manufactories where fire is used.

X.

Experiments on the remarkable Effects which take place in the Gases, by Change in their Habitudes, or elective Attractions, when mechanically compressed. By THOMAS NORTHMORE, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

Devonshire Street, Portland Place,
Dec. 17, 1805.

SIR,

The author's reason for early publication.

IT was my intention to have postponed troubling you with the following experiments upon the condensation of the gases, until I had brought them to a greater degree of perfection; but being informed that several of them have already, by means of which I am ignorant, and probably in a mutilated state, found their way to the press, any further delay seems improper. If then you deem the present communication worthy a place in your interesting Journal, it is entirely at your service.

It

It had long ago occurred to me, that the various affinities which take place among the gases under the common pressure of the atmosphere, would undergo considerable alteration by the influence of condensation; and the success attending the *violent* method adopted by the French chemists, which violence did not appear to me requisite, afforded additional encouragement to my undertaking some experiments upon the subject. His suspicion that the affinities of the gases would be changed by compression.

I communicated this to the late chemical operator in the Royal Institution, a gentleman eminently conversant in the science, and with whom I was then engaged in a series of experiments: he not only approved of my design, but seemed to think it not improbable that an extensive field might thus be opened to future discoveries. Whether these opinions are justly founded, is now left for you, Sir, and the public to judge.

In entering upon a field entirely new, obstacles were of course to be expected: nor without reason; for though I had applied to one of the most eminent philosophical instrument-makers in London, Mr. Cuthbertson, yet I began to fear, even at the outset, that his skill would be set at defiance. The first instruments which he made for the present purpose were, a brass condensing-pump, with a lateral spring for the admission of the gas by means of stop-cock and bladder; two pear-shaped receivers, one of metal of the capacity of seven cubic inches, and another of glass of about three and a half: these were connected by a brass stop-cock, having a screw at each end. The metallic receiver was soon found to be of little or no utility, as well on account of its liability to be acted upon by the generated acids; its being too capacious, and thus consuming too large a quantity of gas: as because, though the result of an experiment might thus be known, yet the changes which the subjects might undergo would necessarily escape observation. The glass receiver obviated all these difficulties, and one or two imperfect experiments were performed with it; but the stop-cock speedily failed in its effect. For the power of the compressed gases was so great, partly from their elasticity, and partly (where affinities had operated) from their corrosive quality, as absolutely to wear a channel in the metal of which the plug was made, and thus

Difficulties of the undertaking as to the instruments.

Condensing-pump.

Receivers.

Various objections.

to effect their escape. But not to trouble you any further with the obstacles that occurred, and which are mentioned only to prevent unnecessary expence to others, I have at last, by Mr. Cuthbertson's assistance, procured a connecting-tube, to which a spring-valve is adapted that has hitherto answered every purpose. See Plate XIV. Fig. 2, 3, 4, 5, 6.

Instruments now used by the author. Pump.

The instruments which I now use, are, 1st. An exhausting syringe; 2d. A condensing-pump, with two lateral springs for different gases; 3d. The connecting spring-valve; and lastly, glass receivers, which should have been of various sizes, but the one mentioned above having burst, that which I have principally used in the following experiments, is of about five cubic inches and a quarter in capacity, and made of glass well annealed and a quarter of an inch in thickness. Besides these instruments, I have occasionally applied Mr. Cuthbertson's double syphon-gage (*See Fig. 6*), by which the number of atmospheres condensed in the receiver, or rather the elastic power of the gases, may be measured; but this is rendered of less service, because a stop-cock must then be placed between the receiver and spring-valve, which frequently impairs the whole experiment; and also because, after a certain degree of condensation, and more particularly upon the admixture of the gases, new affinities usually take place, which tend to diminish the elasticity: the greatest number of atmospheres my gage has yet measured, is eighteen. These, Sir, with some bladders and stop-cocks, various iron screw-keys, and a wooden guard for the legs in case of bursting, constitute the principal part of the requisite apparatus.

Glass receiver.

Syphon gage.

Eighteen atmospheres compression.

I now proceed to the experiments, premising that the first four were made with the imperfect apparatus, when the gas was continually making its escape through the stop-cock.

Experiment I.

Exp. 1. Hidrogen, oxigen, and nitrogen gave water, and probably nitrous acid.

Into the glass receiver, of three cubic inches and a half capacity, were compressed in the following order: Hidrogen, two (wine) pints; oxigen, two pints; nitrogen, two pints.* The result was, water which bedewed the inside of the receiver; white floating vapours (probably the gaseous oxide

* These gases therefore occupied about five times the capacity they were condensed into.—N.

of

of nitrogen); and an acid which reddened litmus paper. Mr. Accum was present at this experiment, and from his opinion, as well as from succeeding experiments, I have reason to think that this acid is the nitric.

Experiment II.

As a difference of arrangement in the order of the gases tends considerably to vary the result, I repeated the former experiment (having first poured a little lime-water into the receiver) by injecting first the oxigen, about three pints, then equal quantities of hidrogen and nitrogen. Much of this gas escaped, owing to the imperfection of the instrument; but upon the affusion of the nitrogen, the white vapours again appeared in the receiver; water seemed likewise to be formed; and some yellow particles were seen floating upon the lime-water. These particles probably arose from the resinous substance, used in fastening on the cap of the receiver, being dissolved by the nitrous gas formed during condensation.

Exp. 2. The same, but the oxigen first.

I would just observe, that the magnet *seemed* to be affected during this experiment; but as there is iron used in the machine, this may be otherwise accounted for.

Experiment III.

Two pints of carbonic acid, and two of hidrogen, were subjected to condensation. The result was, a watery vapour, and a gas of rather offensive smell.

Exp. 3. Carbonic gas and hidrogen. Water and a changed gas.

Experiment IV.

Trying to inflame phosphorus by the condensation of atmospheric air, the bottom of the machine (where it had been repaired) burst out with an explosion. This happened when I had immersed the apparatus in water to discover where the air escaped. The receiver was full of the fumes of the phosphorus, which was itself dispersed in the vessel of water. I afterwards repeated this experiment with the more perfect apparatus, but I could not inflame the phosphorus, and the fumes which arose at first soon disappeared. There was just enough acid (probably phosphoric) formed on the inside of the receiver to tinge litmus.

Exp. 4. Phosphorus in condensed air.

Experiment V.

Exp. 5. Repetition of Exp. 1. with better apparatus.

Having now the ſpring-valve, and new receiver of five cubic inches and a half capacity *, I poured in two ſcruples of ſolution of potaſh, and then injected two pints of hidrogen, two of nitrogen, and three of oxigen. This quantity was hardly ſufficient for the capacity of the receiver, and the reſult was only a ſmell of the gaſeous oxide of nitrogen, a few yellowiſh fumes, and ſcarce enough acidity to tinge the edge of the teſt paper: of courſe, I could not effect the formation of nitrate of potaſh.

Experiment VI.

Exp. 6. Nitrogen (firſt) and then hidrogen and oxigen.

I now determined to begin with the nitrogen, which always appeared to me to undergo the moſt important chemical changes, and therefore injected two pints of nitrogen, three of oxigen, and two of hidrogen. Upon the condenſation of the nitrogen, it ſpeedily aſſumed an orange-red colour, which upon the acceſſion of the oxigen, gradually diminiſhed, and at length diſappeared, though at firſt it ſeemed rather deeper. A moiſt vapour, coating the inſide of the receiver, aroſe upon the compreſſion of the hydrogen, which moiſture was ſtrongly acid to the taſte, coloured litmus, and, when very much diluted with water, acted upon ſilver.

Experiment. VII.

Exp. 7. The ſame, but different arrangement.

Nearly the ſame as the laſt, but with different arrangement. The nitrogen, three pints and a half, was firſt introduced; then the oxigen, two pints; and next the oxigen, three and a half. The nitrogen formed the orange-red colour as before; the hidrogen produced white clouds at firſt (*quære* ammonia ?) which afterwards diſappeared, and the orange-red colour became lighter; but upon the affuſion of the oxigen, the colour did not diſappear as in the laſt experiment, but, if any thing, became darker. I then injected two pints more of hidrogen, but this had little or no effect upon the colour. Some vapour was generated, which was, as uſual, ſtrongly acid.

Experiment VIII.

Exp. 8. Nitrogen over lime-water.

Previous to the burſting of the ſmall receiver, I had put in it a ſcruple of lime, and condenſed upon it three pints o

* One fifth part of a pint very nearly.—N.

nitrogen.

nitrogen. The refult was, a little reddifh colour at firft, which foon vanifhed. Upon repeating this experiment in the large receiver, I could produce no colour at all. In my prefent ftate of knowledge I am unable to account for this circumftance; but as foon as I get my new receivers of a fmaller capacity, I mean to repeat the experiment.

Befides the above, I have made various other experiments with different gafes, but I think it right to repeat them with greater accuracy before I fubmit them to the eye of the public: if upon that repetition they appear to me to be attended with refults of fufficient importance to occupy a place in your Journal, I will take the liberty of communicating them to you, and am, Sir,

Your moft obedient fervant,

THO. NORTHMORE.

P. S. I think it neceffary to add, that during the courfe of the above-mentioned experiments, there was a great variation of temperature in the atmofphere, from the heat of 70 degrees of Fahrenheit to the cold of 33.

Explanation of the Figures, by Mr. Cuthbertfon.

Defcription of a valve for condenfing.

Fig. 2, 3, 4, 5. Plate XIV. reprefents fections of the feveral parts of the fpring-valve for the condenfing fyringe; *a* is a female fcrew, intended to receive the male at the end of the fyringe; *b* is a fquare, to which is a key to fcrew it perfectly tight to the fyringe: *d*, Fig. 3. is a female fcrew fitted to the male *c*; *e* is a male fcrew fitted to the female of the glafs receiver; Fig. 4. is a round fteel arbor, turned with a conical part and flat fhoulder at *a*; *a b* is a fpiral fpring; Fig. 5. is a hollow brafs cylinder ferving as a cover and guide to Fig. 4; the piece Fig. 2. has a fmall hole drilled through the center, and turned out at the end *c*, fo as to fill the cone *a*, Fig. 4; Fig. 3. is turned out at *f* fo wide as to receive Fig. 5.

If the plane fhank of 4 be put into the hole *a c* till its cone fhut clofe into the hollow cone at *c*, Fig. 2. and the other, end with the fpiral, covered by Fig 5. fcrewed tight upon the flat end of *c*, and *d* be fcrewed to *c*, all the joints being properly fupplied with oil, and leathers, it is fitted for ufe.

Fig. 6. Reprefents a fection of the condenfing or double fyphon gage, being a glafs tube bent into the form of the figures,

figures, the end *a* is mounted with a brass screw, having a hole through it corresponding with the inside of the tube, the leg *b c* is filled with mercury, and *d* is hermetically sealed: *d c* is divided into atmospheres.

XI.

Account of a Graphometer for measuring the Angles of Crystals. In a Letter from Mr. Robert Bancks, *No.* 411, *Strand.*

To Mr. NICHOLSON.

SIR,

Great advantage of distinguishing minerals by their figure.

I NEED not point out to you, and to the learned readers of your Journal, how great the advantage will be, whenever the same may be realized, of distinguishing subjects of the mineral kingdom by their external appearance. This has long been done, with considerable precision, by operative men who have acquired their skill from continued practice, but without being able to communicate the knowledge they possess by any simple indications, such as might be given in writing, or through the medium of the press. Neither need I on this occasion point out how much we are indebted to the labours of Bergman, Rome dé l'Isle, and above all, Haüy, for scientific investigations of the forms of crystals, which at present bid fair to afford us criterions of the most extensive use. My present object is to communicate what I hope will be thought one step, however small, towards facilitating the admeasurement of their angles. In your first vol. at page 132, you have given an account of the graphometer of Carangeau, which is now considerably known and esteemed. I have rendered that instrument somewhat cheaper, and easier in the execution, and more correct in its use. For the sake of those who may not have that volume at hand, I shall briefly state, that the instrument consists of a semi-circle, like that which I am about to describe, and a pair of compasses or legs having their centre in the centre of the semi-circle, but capable of having their points drawn back, so as to admit of their application to any small crystals. The arc of the semi-circle is divided

Crystallography.

Carangeau's graphometer for crystals.

divided into two quadrants by an hinge, fo that one part may be turned back out of the way of any mineral, which may require to be brought up towards the centre for admeafurement; and the fame arc can afterwards be reftored to its place, in order to fhew the degree and fraction of the angle.

In my improved inftrument I avoid this joint, and obtain a much firmer framing by making my arc in the form of a protractor, as in Fig. 1. Plate XV. having an hollow centre at A, and a ftud at B, both lying in the direction of that diameter which terminates the graduations. The compaffes, or radii, or legs, are fhewn in Fig. 2. feparate from the arc. Their centre C is made like thofe of the common proportional compaffes, and admits of the legs C D, C F being confiderably lengthened or fhortened when the two pieces are applied to each other. D E the fixed leg is reprefented as beneath F G the moveable leg or radius, and the lower end of the centre pin is made to fit the hole A precifely, at the fame time that the ftud at B being admitted into the long perforation towards E, the piece D E becomes fteadily attached to the femi-circle, as is feen in Fig. 3.

Improved graphometer. The femi-circle is entire, and the compaffes or callipers are ufed feparately for meafuring the cryftal, and applied to the femicircle for reading off.

The ufe is obvious. The cryftal muft be meafured by the detached compaffes as in Fig. 2, which are much more handy for all defcriptions of minerals than Carangeau's entire inftrument; and when thus fet, if fig. 2 be applied to fig 1, as before directed, the angle will be read of at the fiducial edge of G.

I hope you and your readers will confider this as the ufeful fimplification of a valuable inftrument, and fhall be happy to receive your fanction by its appearing in a work fo generally known and efteemed as your Journal.

I am, Sir,

Your obedient Servant,

ROBERT BANCKS.

Nov. 1, 1805.

ACCOUNT OF NEW BOOKS, &c.

Philosophical Transactions of the Royal Society of London for 1805. Part II. Quarto 353 pages, with an Index, and Six Plates. Nicoll.

Philosophical Transactions of the Royal Society.

THIS part contains the following communications, 1. Abstract of Observations on a Diurnal Variation of the Barometer between the Tropics. By J. Horsburgh, Esq. 2. Concerning the Difference in the Magnetic Needle, on board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, Esq. Commander of his Majesty's Ship, Investigator. 3. The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uses in different Animals. By Anthony Carlisle, Esq. F. R. S. 4. On an Artificial Substance which possesses the principal Characteristic Properties of Tannin. By Charles Hatchett, Esq. F. R. S. 5. The Case of a full grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, F. L. S. 6. A Description of Mal-formation in the Heart of an Infant. By Mr. Hugh Chudleigh Standart. 7. On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Esq. F. R. S. 8. On the Direction and Velocity of the Motion of the Sun and Solar System. By William Herschel, L. L. D. F. R. S. 9. On the reproduction of Buds. By Thomas Andrew Knight, Esq. F. R. S. 10. Some Account of Two Mummies of the Egyptian Ibis, one of which was in a remarkable perfect State. By John Pearson, Esq. F. R. S. 11. Observations on the singular Figure of the Planet Saturn. By William Herschel, L. L. D. F. R. S. 12. On the Magnetic Attraction of Oxides of Iron. By Timothy Lane, Esq. F. R. S. 13. Additional Experiments and Remarks on an Artificial Substance, which possesses the principal Characteristic Properties of Tannin. By Charles Hatchett, Esq. F. R. S. 14. On the Discovery of Palladium, with Observations on other Substances found with Platina. By William Hyde Wollaston, M. D. F. R. S. 15. Experiments on a Mineral Substance, formerly supposed to be Zeolite, with some Remarks on Two Species of Uran-glimmer. By the Rev. William Gregor.

Transactions of the Royal Society of Edinburgh (being the Continuation of Part II. together with Part III. of the Fifth Volume) Edinburgh Quarto 100 pages Continuation of Part II, and 126 Pages, Part III. No Plates. Transactions of the Royal Society of Edinburgh.

THE heads of memoirs and communications made to the Society since their last publication are disquisitions on the origin and radical sense of the Greek prepositions, by Mr. James Bonar, and experiments on the contraction of water by heat, by Dr. Thomas Charles Hope. These two papers of which the latter is inserted in our Supplement, complete the second part: and the third part contains the history of the Society consisting of the following articles. 1. Of the Diurnal Variations of the Barometer, by Mr. Playfair. 2. Aurora Borealis observed in Day-Light, by the Rev. D. Patrick Graham. 3. Phenomenon of Two Rain-Bows intersecting one another, by Mr. Playfair. 4. On the Combustion of the Diamond, by Sir George Mackenzie, Bart. 5. Remarks on the Basalts of the Coast of Antrim, by the Rev. Dr. Richardson. 6. Rule for reducing a Square Root by a continued Fraction, by James Ivory, Esq. 7. Singular Variety of Hernia, by Mr. Russel. 8. Concerning the Chartreuse of Perth, by the Abbé Mann. 9. Explanation of the Old Word Skull or Skoll, by the Rev. Dr. Jamieson. 10. Biographical Account of the late Dr. James Hutton, by Mr. Playfair. 11. Minutes of the Life and Character of Dr. Joseph Black, by Dr. Ferguson. 12. Appendix List of Members elected since the Publication of the last Volume. 13. List of Donations.

Academical Institutions in America.

THREE Institutions for the promotion of Natural Philosophy and the Arts, having been established in the united states of America, not many months ago, of which no notice has hitherto appeared in this work, it is hoped the following account of them will not be unacceptable.

The first is an Academy of the Fine Arts, of which the first idea is due to Mr. Livingston: The public were so sensible of its importance, that long before the arrival of the

 plaister

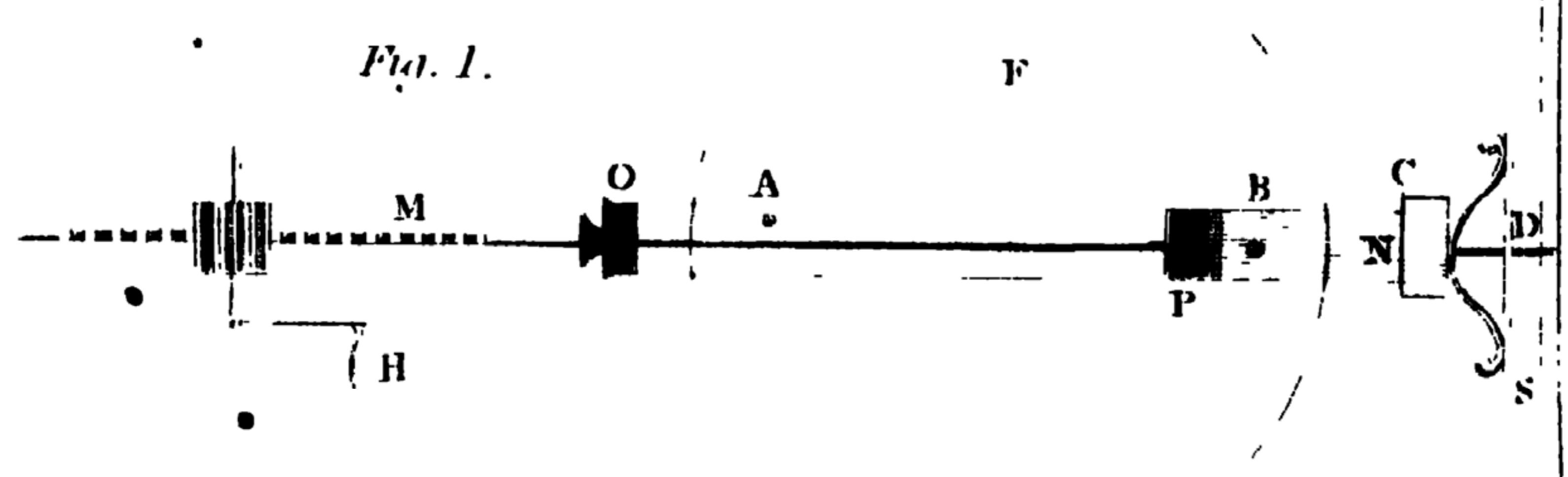
plaister of Paris casts, which he presented to the infant Society, the number of subscribers, at 25 piasters each, amounted to 180.

The second Institution is a Botanic Garden in the neighbourhood of New-York; as yet but a small part of the treasures of the vegetable kingdom are to be seen in it, but the admirers of botany hasten to send to it every interesting plant which is to be found in their vicinity. The charter of incorporation of the subscribers, is entirely conformable to the views of the founders of this garden of plants, and according to custom, ensures the permanency of the establishment: when the hot houses are finished, it is expected, that the collection of every thing rare and most interesting, produced by the southern states, will be compleated.

The third Institution is an agricultural society, established at Washington, under the special protection of government. The president of the United States, who is a most enlightened agriculturist, the chief men of the administration, the senators, and the deputies of congress, are all members of it officially. The society being now wealthy from the sums granted by government, and the numerous subscriptions of associates and correspondents, have purchased an handsome house, and a farm of thirty acres; they have also began a library; and are in possession of the fine collection of ploughs, and other instruments of Agriculture, which formerly belonged to general Washington: the form of its administration, the number and the succession of its members, the capital which it may possess (specified in bushels of corn) and its whole organization is regulated by its charter of incorporation; which constitutes this association a body politic, and fixes the perpetuity of its continuation: It is reported, that the answers which it returned to the numerous questions proposed by the societies of the different states soon after its establishment, will form a very interesting work which will soon be published.

Mr. Wright's new Air Pump.

Fig. 1.



Mr. Cuthbertson's Valve for a Condenser &c.

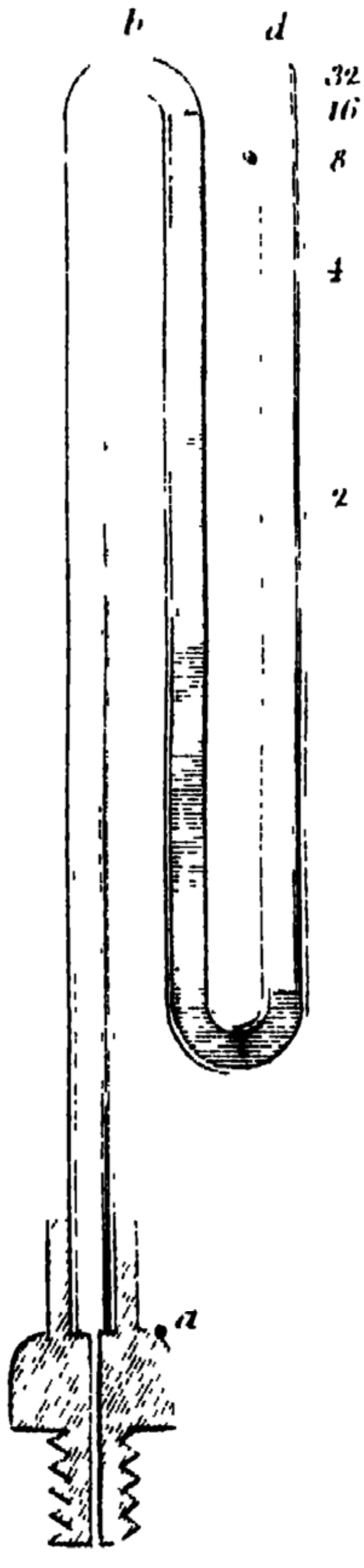


Fig. 6

Fig. 2

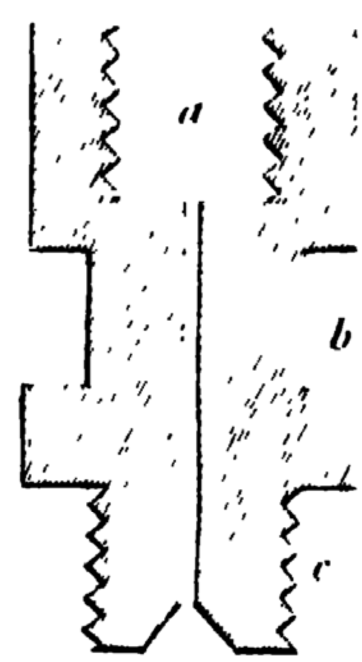


Fig. 4

Fig. 5

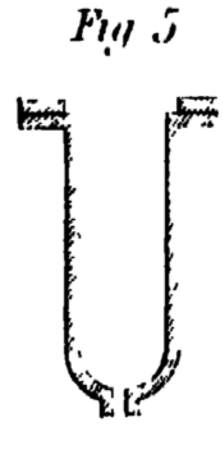
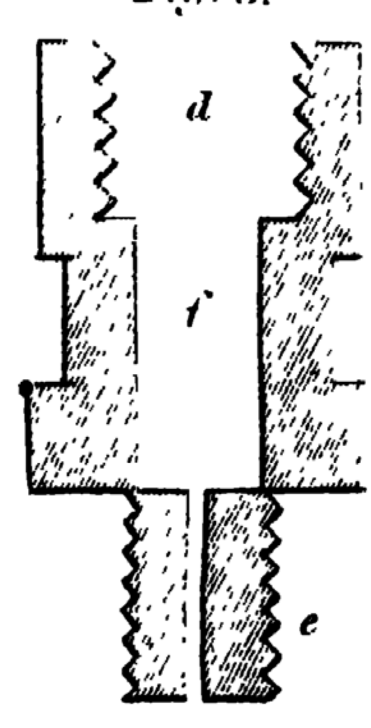


Fig. 3.



Improved Graphometer by Mr Banks

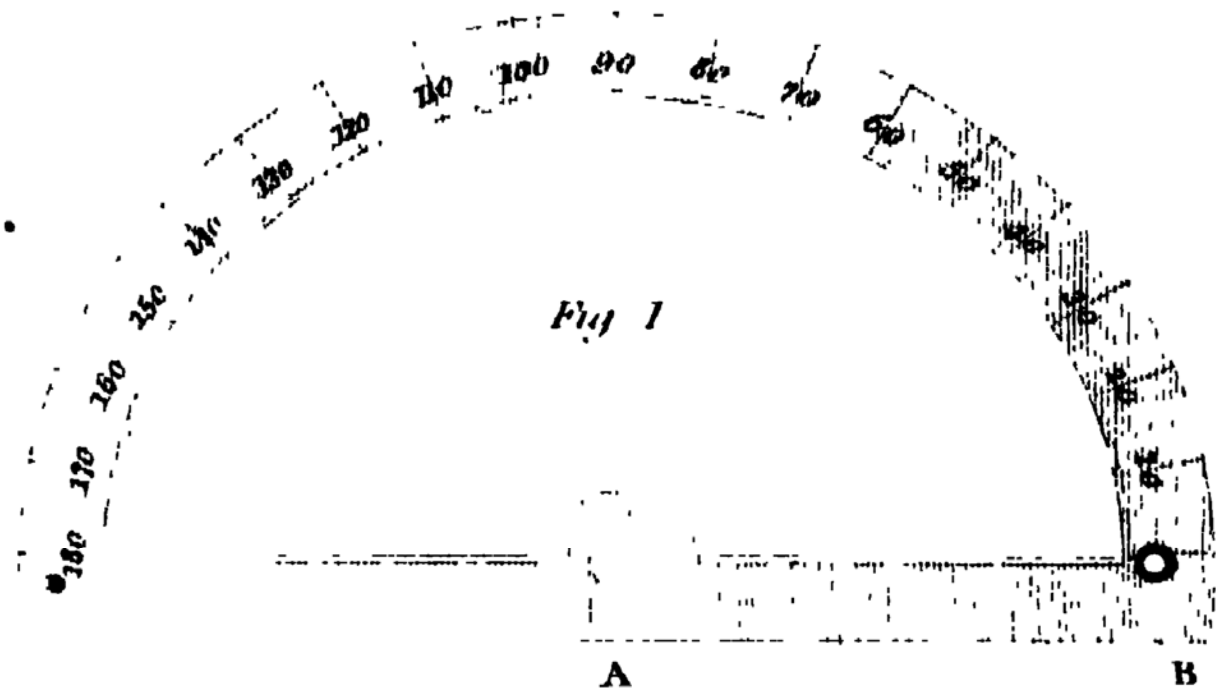


Fig 1

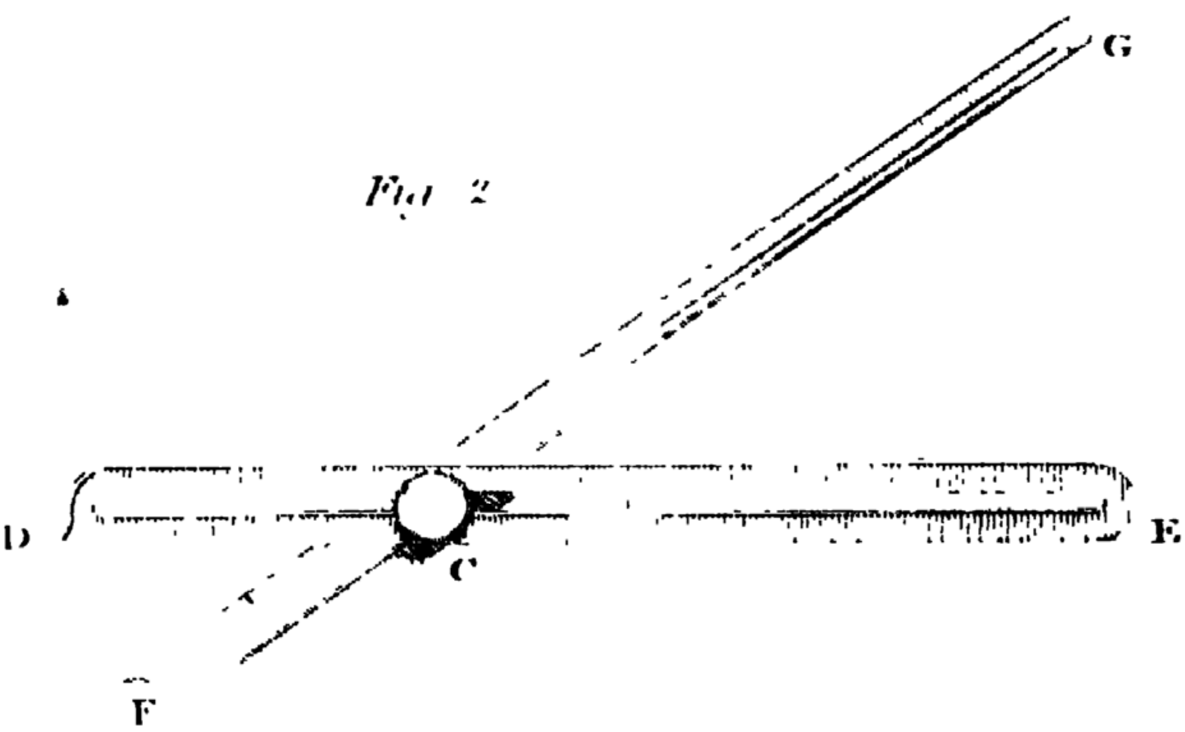
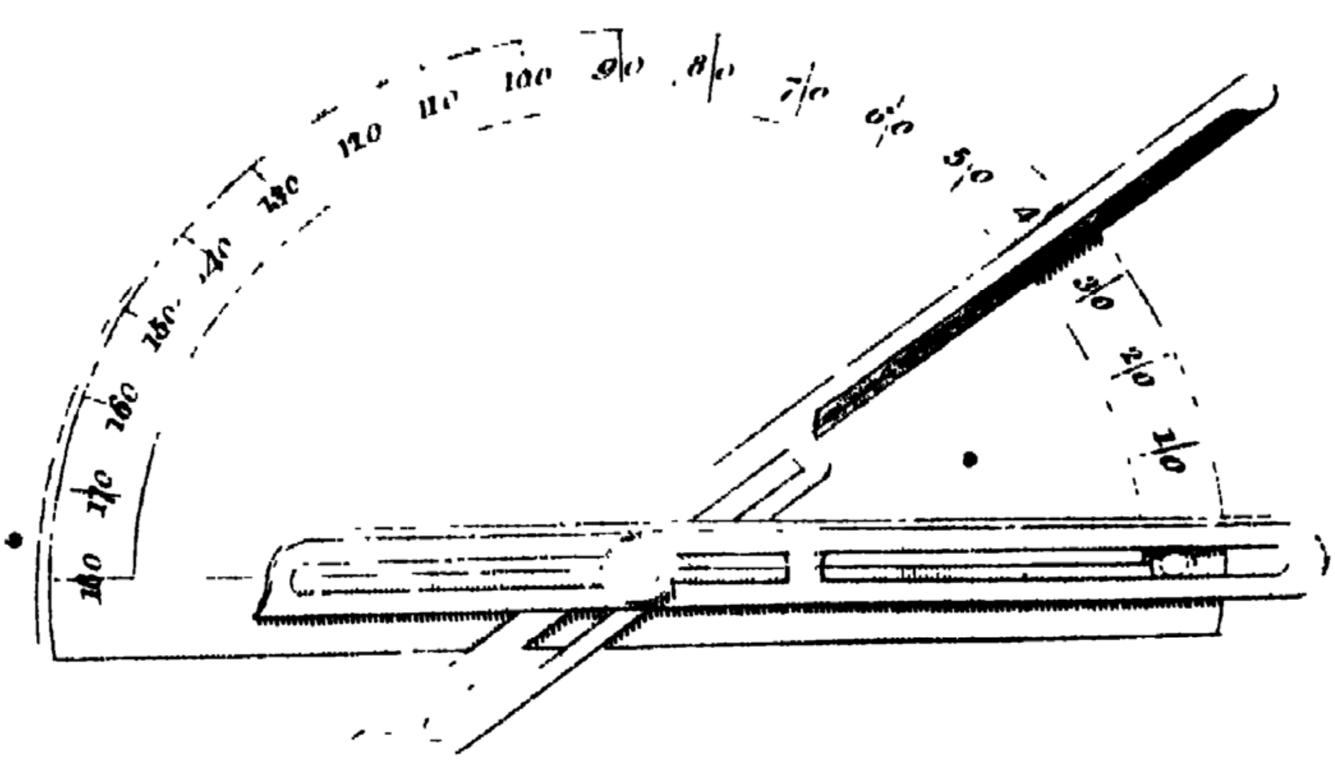


Fig 2

Fig 3



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Criterion of Life.

DR. STRUVE has contrived an apparatus which is mentioned in the foreign Journals, but not described. The object of its application, is to shew by means of galvanism, whether the appearance of death be real; a purpose sufficiently interesting to every human being, who has for a moment reflected on the satisfaction which recovery from apparent death must give to the friends and relatives of the individual supposed to be dead; and on the still more impressive and dreadful incident of recovery after burial. Our galvanic and anatomical philosophers will find no difficulty in applying this powerful agent to so good a purpose, in which the learned Doctor has the merit of taking the lead. Criterion of Life.

Mr. SESSKEN who has successfully laboured in the construction of a reflecting telescope of thirteen feet focus, has lately supplied the Observatory at Lilienthal, with two mirrors of fifteen feet focus and eleven inches aperture, which prove to be excellent, and bear the magnifying power of 2000 very well, on the proper objects, and at such seasons as are fit for making observations of this nature.

Numbering of Houses.

A NEW mode of numbering houses has lately been adopted at Paris, which is attended with much advantage, and deserves to be followed in this country. Over each door the numbers are painted in large distinct characters, and in conspicuous colours; they are generally either brown or red, on a yellow ground, surrounded by a blue square; but the principal singularity, which is the object of this notice, is, that all the odd numbers are placed at one side of the street, and the even numbers at the other; by which means, may be seen at once on entering the street, at which side of the way the house is, which is sought for; by which much time may be saved, not only by its making it unnecessary ever to cross the street more than once, but also by its always Numbering of houses.

preventing

preventing the trouble of returning back again on the other ſide of the ſtreet from that already paſſed, to find a particular number, which often happens, where the old method of numbering is uſed, from the order of the numbers proceeding regularly down one ſide of the ſtreet, and back again in the reverſed direction on the other; and which, when the ſtreets are very long, as many are in this metropolis, is often attended with ſerious inconvenience; but in the new method of numbering, this can never occur, as in it the numbers proceed in the order of progreſſion in the ſame direction at both ſides of the ſtreet.

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Corneen

Indostan,

T.

Thomas

THE END OF THE TWELFTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar.